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THE PSYCHOLOGICAL REVIEW

ASPECTS OF LEARNING[1]

BY JOSEPH PETERSON

George Peabody College for Teachers

Why do we see objects about us right side up despite the fact that their images on the retinæ are inverted? Probably one of the most naive answers to this question is that these retinal images become righted by the time they get to the "center of vision." The images, of course, never get there; and if they did, there would be nothing there to see them! The view of the layman that we see the objects "out through our eyes," or hear them through the ears, is just as inaccurate, as we all know. Objects on which the eyes are focused, as the stereoscope readily reveals, are seen from a point of view between the eyes and the double images of normal vision are disregarded. Auditory localization is likewise an immediate 'thereness' ordinarily not associated with the ears at all. The experiments by Stratton (**67–71**) and, more recently, Ewert (**18**), investigating the effects of reversing the retinal images by means of appropriate lenses which the subjects wore continuously for a period (in the latter case) of approximately two weeks, indicate that the great disturbances due to reversing the apparent position of the visual objects become gradually corrected if the subject is active during the experimental period. Ewert, on three subjects, studied a number of detailed overt localizing responses (with both hands and feet) to visual objects whose retinal images had been thus reversed; and he obtained gradual improvement in accuracy, the results

[1] Presidential address delivered before the American Psychological Association, New York City, September 7, 1934.

yielding ordinary learning curves of decrease in errors or time. Certain exceptions in the case of auditory-manual localizations outside the visual field were to be expected. Such activities were not affected. The unfortunate inversion of distance, as well as of right-left and up-down relations, brought about by the lenses introduced unavoidable internal disturbing factors of inconsistency which interfered with the study of general orientation changes. Unless enough time for continuous experience with the inverted retinal images is given for adequate adjustment of all such inherently contradictory elements as reversal of depth factors as well as of two-dimensional space factors, the value of such general orientation studies, so far as our present purpose is concerned, is largely lost. There seem to be no *a priori* reasons for holding that such contradictory factors would not in time yield to the demands of consistency so that overt responses to visual or auditory stimuli would under such continuous experience become entirely harmonious with tactual-motor factors. Young's experiment (**100**) with the pseudophone was suggestive as to the possibilities of one's becoming habituated to the interchange of right-left auditory receptors. The early part of the experiment, however, lasted only one hour, and later two hours, per day. When finally the subject did not receive any auditory stimuli except through the pseudophone for a period of three days, vision was not excluded and was, indeed, often used explicitly to check up on the accuracy of auditory localizations.

But even with these shortcomings and difficulties in these experiments with visual and with auditory disorganizations—some of which could not have been adequately guarded against in advance of considerable preliminary work—many useful bits of information for further experimentation are now available. In a number of cases in the visual experiments, interesting tendencies to inversion effects remained when the subjects went back to normal conditions of stimulation (**68**, **69**). Such tendencies have often been remarked spontaneously by unsophisticated college students after carrying out star-tracing experiments before a mirror for an hour or more. For com-

parable after-effects in audition, Young's experiments were too short and too much interrupted. The changes in orientation effected during the periods in which the disturbing visual apparatus was worn and the tendencies to disturbance after the removal of the lenses, gave clear indications that overt adjustments to objects about us—even our 'perceptions' of these objects—*are learned reactions*, acquired perhaps by the usual methods of 'trial and error' learning. Further experiments are needed, however, if we are to solve some of the most important psychological problems still open in the field of perception.

It is well known that objects below the line of sight stimulate the upper part of the retina and that those to the right stimulate the left part, and correspondingly for higher and leftward objects. There is no apparent reason why such reversed stimulation effects should not be changeable with long use of lenses which would bring about stimulation by objects on the same side of the retina that the objects occupy. Stated in this manner the logical basis for adaptation to reversed images on the retina seems very clear, but it must be remembered that the lenses make distance reversals so that far objects tend to seem near, and near objects far; moreover it is essential that our views be based squarely on indisputable, empirically derived data and not merely on logic. One recalls the old arguments for the transmission of acquired characters in heredity. It would seem desirable that experiments on the effects of the reversal of retinal images should be carried out in part at least on persons who are deaf, and that auditory experiments with reversed receptors should employ blind subjects, if effects of habituation to these reversed conditions are really to be studied scientifically. Language reactions should be entirely ruled out during the experimental period so that there would be no tendencies to revert constantly to old systems of orientation. Deaf mutes for the visual experiments would in many ways be ideal. The subject should in every case be required to go and touch or handle the stimulating object. Plans are being made for carrying out such experiments. The experiments by Stratton and by Ewert with the inversion of retinal images, and those by Young on reversed

auditory receptors, reveal clearly that there are inconsistencies between these two distance receptors, when reactions to either visual or auditory stimuli are investigated, and that these conflicts are slow to resolve themselves. In Young's experiment, front and back reversals were often experienced in fields outside of vision or with eyes closed; because, evidently, right and left reversals would be inconsistent if there were no corresponding changes in front and back directions. The experiments proposed could be made wholly objective, requiring no verbal or other descriptions before the completion of the experiment. The reason for this procedure is the necessity of avoiding any reactions that would tend to throw the subject back upon the old system of orientation while the new system is being developed.

If it can be shown, as seems probable from data already available, that the factors of auditory localization in normal hearing—namely, earlier phase of the air-wave (**59**, **95**, **20**, **2**), greater intensity (**95**, **2**), and greater complexity (**1**) in the ear nearer the sounding object [2]—can by habituation be made to lose their effects, and that lateness of phase and lower relative intensity and complexity can be substituted for them in correct localization, and work as well, then we shall have made a most significant addition to our knowledge toward the solution of disputed points of view in psychology today. When Lord Rayleigh (**59**) proved in 1907 that a constant, relatively earlier phase of the sound-wave in one ear than in the other (with such limitations as to wave-length as are necessary) is sufficient to cause the sound to be heard continuously in the ear receiving the earlier phase, Wilson and Myers (**95**) suggested that this simple fact, if finally established, would change seriously our notions of sensory-nerve physiology. They tried to explain the effect as due to greater intensity in the near ear, but their basic assumption of bone conduction was found untenable in Moore's (**39**) and in my experiment (**45**) on a subject in each case who was totally deaf in one ear. The earliness of phase [or of temporal order, if with Banister (**2**)

[2] It is well known that air waves of high frequency are interfered with, or are actually obliterated, in rounding the head.

you care to emphasize that] has been finally established as a factor, it seems (**2**, **95**), even though the resonance hypothesis has even been strengthened of late years (**38**, **94**, **19**). Now if habituation to the pseudophone will entirely reverse the directional auditory cues so that all three of these (phase, intensity, and complexity of the wave) will work just as effectively in the reversed conditions, and especially if the normal orientation will gradually reappear when the pseudophone is discarded finally and if similar changes of adaptation to upright images on the retinæ are found possible, and later when the lenses are discarded if readaptation to the normally inverted images can be indisputably established (even if it should take months to effect these adaptations), we shall have proof that the factors of visual and auditory direction orientation are organismic in the larger sense, involving the welfare of the whole individual, and not specifically "necessary" in the manner of operation now accorded them in psychology. It would show that the operation of any such specific factors as we now hold to be necessary in auditory and in visual localization are effective *only in a habituated or learned situation*, but that the real determining factors are *functions of the subject's recent behavior under specified conditions;* that there is nothing "necessary," generally speaking, about them. The importance of such an experiment is emphasized here, because it would seem to be crucial on certain essential points between nativists and empiricists, and between organismic views of learning and the views which regard learning as a mere weaving together by association or conditioning of separate "right" acts through the elimination of "wrong" acts regarded also as disparate units. I have personally favored the organismic view[3] for years but have recognized, and still recognize, the futility of using the expression "the organism as a whole" as an explanatory category for the same reason that I must reject "insight." These terms are in themselves not explanatory.

There seems no more to be any very satisfactory evidence to support the old view of certain early English empiricists, especially Locke and Hartley, that we perceive things in the

[3] Humphrey (**29**) has ably presented the organismic theory of learning.

so-called "outer world" merely on the reception of sensory or afferent impulses. Cattell (**5, 6**) on the basis of a variety of experiments and observations, including reaction-time experiments, early came to the view that perception does not follow immediately in a passive, receptive sense the arrival at the sensory cortex of mere exteroceptive impulses, but that it results from, or is an aspect of, the *response* to the stimulation; that it is a motor, rather than a sensory, phenomenon. Dewey (**13**), in his famous article on the reflex arc concept in psychology, likewise emphasized the selective activity of the organism in perception and the error of separating the sensory and motor phases as to temporal order. My own attempts at learning-curve plotting (**50**, pp. 218 *ff.*) showed that by a different method of plotting one of Yerkes' *sensory* discrimination learning-curves (**96**, p. 230), a so-called typical *motor* learning curve was obtained. There still seems to be need of careful analyses of the learning processes called either sensory or motor. It is probable that any such alleged one-sided process may just as easily be regarded as belonging to the opposite aspect of the process. If a dog is conditioned, for example, to salivate when a tone is sounded in connection with feeding, we may say either that a new auditory (*sensory*) inlet has been made an effective stimulus avenue to the same response that formerly only the gustatory inlet would bring about; or that a new sort of *response* has been acquired to the sounding of a tone. Washburn (**88, 90**), Dunlap (**17**), and Langfeld (**33, 34**) have stressed the motor aspects of perception, and have presented views of various other writers with supporting data; and Judd (**30**) added a significant chapter on "Experience and Expression" in his "Psychology" published in 1907, in which he stressed the motor elements in both feeling and perception. That perceptions are definitely selective reactions is now not difficult to establish; most experimental psychologists have had these facts strongly impressed upon them. It is only by an unguarded accident of statement that a psychologist can say, as one said in a recent popular book (**37**, p. 17), that "at one pause, and while holding still they (the eyes) recognize or take in two or three words," etc. The

discussion related to eye movements in reading. Of course, *the eyes* do not see or recognize; the *individual* does. Moreover, it is important to note that he sees most accurately in situations where his reactions to visual stimuli have had opportunity of being most accurately and immediately checked as to correctness or adequacy. There can be no doubt at all to experimenters in learning, that the *results* of the responses of an animal or human being modify these responses so as to eliminate the ill-adapted and inaccurate aspects of the behavior. Thorndike (**74, 75**) was one of the early psychologists to emphasize this truth. We shall come later to a more direct consideration of his "law of effect." Tolman (**72**, Parts II and V) and his students have furnished valuable data on the effects of changes in results which follow certain responses in learning.

Psychology has been slow to recognize the close relations between "perception" and "learning," largely because these terms have been emphasized and brought to the foreground in different historical periods when different experimental methods and points of view prevailed. The perception of objects was considered by Locke, and a generation later by Hartley, largely as a receptive process; but Berkeley (**3**) in 1709 at the early age of 24, also and more directly stimulated by Locke, made a keen analysis of visual space perception in which he found that various sensory factors, including sensations of strain of the muscles of accommodation and convergence, served merely to "suggest" tactual-motor experiences. "I believe," he said, "whoever will look narrowly into his own thoughts, and examine what he means by saying he sees this or that thing at a distance, will agree with me, that what he sees only *suggests* to his understanding that, after having passed a certain distance, *to be measured by the motion of his body, which is perceivable by touch*, he shall come to perceive such and such tangible ideas, which have been usually connected with such and such visible ideas" (Art. 45). While he over-estimated the sensory factors in the operation of the muscles concerned in movement, he stressed the fact that perception is learned and that it is based on "habitual con-

nections," not on "necessary connections." Much of his suggestive work was lessened in importance, especially to his own country, by the inferior work of the Scottish intuitionists, like Thomas Reid and Thomas Brown, who emphasized the immediate givenness of the objects in our sensory experience.

The experimental psychologists, particularly in Germany, established, through careful and patient experiments, the facts that tactual localization or discrimination is more accurate crosswise than longitudinally on the limbs or body, more accurate also for successive than for simultaneous stimulation, and on the readily movable than on the less movable parts of the anatomy. Experiments had also shown that fatigue decreases and practice increases discriminability, but that this gain in accuracy is rather rapidly lost with cessation of practice. Considerable mystery has been attached to the "local signs," since it seemed necessary, in accordance with the sensory psychology of that day, to regard the "local signs" as some sort of sensory *qualia* which must be attached to the various sense modalities themselves but which are neither analyzable nor detachable from these modalities. Baldwin's "Dictionary of Philosophy and Psychology" defines "local sign" as "a specific 'shading' or 'coloring' of certain sensations whereby every such sensation is invariably referred to a definite position upon the organ or elsewhere in space." [4] Undoubtedly, as I have suggested elsewhere (**55**), local signs are reducible to orientation tendencies toward stimulating objects, tendencies which are not "guided" in any true sense either by any sort of remote imagery or by kinæsthetic sensations. This view relates perception directly to the general field of learning and makes clear to us why our perceptions are more accurate in fields where activity is most easily checked up. Renshaw and his students (**16**, **60**, **61**, **62**) have obtained marked improvement with practice in tactual localization on both adults and children. Perceptions are apparently reactions which change with new demands of the environment on the individual, just as all other habitual reactions do.

The psychological theory of associationism, whether ap-

[4] This is, of course, a more definitely sensory view than that which Lotze held.

plied to "ideas" or to reflexes as units—even Pavlov (**44**, pp. 91–97) has repudiated the psychologists' wholesale use of conditioning—has been prone to emphasize unduly a mere passive repetition for the fusion of its units and to attribute constructive learning to the effects of assumed original powers usually called "attention" or "will." Under the dominance of that view repetition was the general panacea for learning as well as for retention. Each stimulus, moreover, was supposed to be effective rather immediately and momentarily so that learning was thought of rather generally as the making of connections between serial stimulus-response units. This view had raised so many difficulties in my own thinking and teaching that, after a few years of study of the problem, I formulated in 1916 a tentative hypothesis (**46**) called the "completeness of response principle." It was only an hypothesis to be tested out by certain experiments which were briefly indicated.

The hypothesis was of course influenced by the current literature on learning and was immediately stimulated by the then recent discussions of selection principles in learning by Watson (**91**, ch. 7) and Carr (**7**). It was based on assumptions of (1) overlapping of impulses both from present and recently past stimulation (inner and outer) and from responses, (2) mutual facilitations and inhibitions among these various impulses, those more consistent with one's organismic and environmental conditions being successful over the less consistent ones, (3) the influence of certain metabolic processes and sets or attitudes later called "drives," and (4) the gradual release of the hold-over effects when certain successes were attained as in "consummatory responses," these responses being "more *complete*, or unitary," than the other tendencies which were inhibited. Pleasure and displeasure, and even pain, were not regarded as causal factors or stimuli. The general idea was, then, that the organism, not the separate responses, is the unit; that, by means of various inner mechanisms, hold-over effects of the impulses and attitudes from recent stimuli and responses occur. These persistent effects thus become operative simultaneously with present external

and internal stimuli and bring about reactions giving the greatest completeness of response to the organism according to the general consistency of these various impulses.[5] A previous study of illusions of orientation (47) had yielded evidence that psychologists had been too neglectful of certain larger orienting attitudes in learning.

The experiment (48) to test the hypothesis was planned to see whether short blind alleys would be eliminated more readily than long ones since the latter would give more time for the loss of tentative reactions and attitudes. Twenty-four rats were used in two different mazes. In one maze half of the blind alleys were shortened for one group of animals and the other half for a different group, while in the other maze there were no shortened blinds for one group of rats and all were shortened for the other. Results showed that in the first maze 608 entrances were made into the longer blind alleys against 325 (or 53 per cent as many) entrances into the shorter ones; and that in the second maze the entrances into the long blind alleys were 1311 against 929 into the short ones, a decrease for the short blind alleys of 29 per cent. Since each entrance was an error, results showed that 1919 error-runs were made in the long blind alleys, whereas only 1254 (or 65 per cent as many) were made in the shorter blinds. The data, then, supported the hypothesis, since the rate of elimination of entrances to blind alleys correlates highly with the number of entrances into them.

Since the number of animals was small in comparison with the number now usually used, it is desirable that the experiment be repeated in more uniformly shaped mazes on a much larger group of animals. The experiment incidentally yielded information on other important aspects of learning and particularly on the method of elimination of errors about which no previous exact data were available. The rats were noted in

[5] Warden (87, p. 55) has made a fairly accurate statement of the "completeness of response principle," but he has not emphasized sufficiently the role of objective stimuli in my own conception of the principle. Wiltbank, Kuo, and others have shown a similar shortcoming in interpreting and criticizing it. This may reflect a failure in the early statement of the view to make clear the role of objective factors, due probably to a neglect to emphasize the obvious.

many cases—especially in long blind alleys—to eliminate erroneous entrances by degrees, beginning with full entrance and gradually shortening the degree of entrance till in recent stages only short, quick head-vibrations occurred. At this stage of the learning an erroneous choice would occasionally result in a complete entrance with a crash into the end of the blind alley, especially when this blind led off in the same direction as the true path. Such an error was found in one case (**48**, p. 28) so to disorient the animal to the whole maze situation that several errors in other parts of the maze, formerly eliminated, were made. These disorientation difficulties persisted for a few trials afterwards. This larger disorientation resulting from a particular error was interpreted to indicate that the animal was becoming gradually oriented to the entire maze situation with respect to the food box, as Hubbert and Lashley (**24**) had previously found in the circular maze. Curves of rate of elimination of return runs from blind alley exits (**48**, p. 24), as compared with curves of elimination of entrances into the blind alleys show a strong tendency in the rats to learn relatively early to keep a forward direction in the maze after erroneous choices. At first the untrained rat shows strong tendencies to return frequently to the starting point, tendencies which doubtless have a survival value early in youth. A detailed record of all the responses of the animals in this initial experiment led to a further study (**49**) of the effects of recency and frequency factors.

It had been noticed in the investigation just described that errors were much more frequent in the blind alleys near the entrance to the maze than near the goal and that their frequency decreased toward the goal or food box. Watson (**91**, ch. 7) had shown that on probability principles alone a rat had a chance of 3 in 4 of passing successfully any blind alley encountered, and of only 1 in 4 of being turned back by it. This suggestion was helpful, but it was noted that on any backward run the chances were reversed, being 3 in 4 of continuing in the reverse direction and only 1 in 4 of being properly reoriented at any blind alley. A forward and backward run over any section of the maze would therefore give equal fre-

quency at any blind alley passed of forward and backward runs from it, thus affording no opportunity for learning at all. Watson had failed, however, to note one significant fact in favor of his view: that every trial must end with one extra forward run past every blind alley because whenever the food box was entered the animal was not permitted to return, this fact being wholly independent of any effect that the food might have. The discovery of this fact led to an experiment with the choices and maze progress of "imaginary" rats following pure probability principles. "Choices" at each bifurcation encountered were determined by the flipping of a coin. "Heads" always designated favorable choices and "tails" unfavorable ones. In the forward direction each "head" was recorded as a run past a blind alley and each "tail" as a run into it. On emergence from any blind alley forward turns were recorded for "heads" and backward turns for "tails"; and on return runs "heads" were recorded as entrance to blinds, "tails" as continued return runs. From these pure chance data, tabulated in the order of the flipping of the coin, it was definitely established that errors due to entrance to blind alleys occur most frequently in the early part of the maze and gradually decrease toward the food box. This distribution of errors was clearly due to the fact that all runs began at the entrance point of the maze and that all entrances to the food box ended trials and thus prevented return runs from that end of the maze, while returns to the place of entrance into the maze had always to be followed by further forward runs for the trial in question. These chance results were verified by the actual distribution of the errors made by the animals in the four mazes, and they agreed with observations by Carr (**8**) made independently during the same year. Reports of both of these studies were published in 1917, but my own interpretation of the results was new.

It was noticed by both investigators that many factors—previous training, individuality of the animals, the different forms of the mazes used, etc.—slightly disturbed the order of error-distribution based on pure chance expectancy. For this reason, it was found desirable to construct what has been

called a mental maze (**52**), so that spatial factors would be entirely eliminated. This new maze was used on adult subjects. Different parts were lettered and the experimenter called out successive pairs of letters, requiring the subject to make such a choice from each pair as would seem best for solving the problem. He was informed in each trial just when he reached the goal, and also as to the total number of errors made in the trial, but not where the errors were made. Whichever letter in a pair was called first was determined by a chance schedule. Precautions were taken against any subject's following a particular scheme, such as trying to spell certain words, which would make learning impossible, and all responses were recorded in the order of their occurrence so that at any stage of learning the objective situation and the subject's previous responses would be exactly known. This experiment employed 19 college students as subjects, each continuing until there were two trials in succession without any error. Results showed clearly the accuracy of the previous observations as to the distribution of errors mentioned above, but individual differences, probably due to preferences for certain letters over others and to other such minor factors, still disturbed somewhat, as might be expected, the order of the distribution of the errors. There was, however, a general decrease in errors from the first blind alley encountered to the tenth, next to the goal, and backward elimination of errors.

By mathematical calculations based on chance factors, a table was constructed (**52**, p. 273) to show the number of times per trial that an animal or human subject would make an error of entrance into each blind of a ten-blind-alley maze, and also the number of times that the subject would pass each blind alley in the forward direction. The calculation is based on the assumption that the subject is free at any stage in the maze to make complete or partial returns to any earlier part of it. While this table is too complex to consider here, an illustration of its use may be given to make clear the general explanation of backward elimination of errors in the maze. In a ten-blind-alley maze, for example, with no prevention of return runs from any blind alley, the ratios of forward runs

past the successive blind alleys to forward runs into them in a single trial by untrained animals or human subjects are as follows from the first to the tenth blind: 1.03, 1.04, 1.05, 1.07, 1.09, 1.11, 1.15, 1.19, 1.25, 1.33, thus making an advantage of .30 for learning at the tenth over learning at the first blind alley encountered. These ratios, which, it will be noticed, increase in an accelerating fashion from the first to the last part of the maze, have been called "learning coefficients" (52, p. 277). An even more concrete illustration of this explanation of learning in the maze follows: if the subject has passed all blind alleys but the last one, there is an even chance of going directly into the goal or into the blind alley. Entrance to the goal would be the consummatory response from which there would be no return, so the trial would end. But entrance into the blind alley would still permit a chance of 1 to 1 of going directly into the goal on emergence from the blind alley, making altogether a chance of 3 in 4 of entrance into the goal. This chance is wholly independent of any other goal effects, or of any "attraction" by food, as some investigators put it, and it holds as well for motivation by electrical punishment of animals for wrong choice or by instruction to human subjects as for motivation by food or other rewards. Now, it must be noted also, that when the subject under these advantages has learned to eliminate the tenth blind the same advantage that existed there comes to the ninth blind, and so on back to the first one. This, then, it seems to me, is the real reason for the backward elimination of errors in maze learning usually found empirically to occur, and this explanation is not in itself affected by the position of the goal in the maze or by whether the true path is relatively straight or tends to curve back toward the entrance region of the maze, however much other minor factors, like general forward momentum, odor of food, general orientation toward the goal, etc. may count.

Again, since the forward runs past any blind alley will always exceed the return runs from it by unity (because the final run into the goal permits no return), the equation

$$f - 1 = b$$

or in a different form, before b becomes zero,

$$f/b = 1 + 1/b$$

holds when f is for forward and b is for backward runs. The equation has only one constant term, unity. Now, since the actual values of f and b constantly decrease from the beginning toward the distal part of the maze, it is evident that the ratio of f to b must constantly increase, with the greatest value coming at the last blind alley.

The foregoing explanations, then, seem sufficient to account for the backward elimination of maze errors. It has been shown conclusively on the mental maze (**53**) that if the subject is thrown back to the beginning point each time when any error occurs, the elimination of errors proceeds in the *forward* direction, and the reasons for this reversal in learning under these conditions have been made clear in the article cited. This easily effected reversal with complete throw-back for every error is another substantiation of the explanation of backward elimination here emphasized. The question has frequently occurred to me, and will not stay down, as to whether this general explanation (which seems to be indisputable as applied to mazes) will not also apply, in many if not all types of constructive learning, to the nerve functionings in the organism, consummatory responses being regarded as comparable to goal entrances in mazes. It is conceivable that certain experimental set-ups might test out the suggestion. Such an experiment would have very great value if it could throw any light whatever on the difficult problem of the neural basis of learning.

But since the explanations discussed in the immediately preceding pages are based largely on chance laws, can we suppose that the greater probability of runs past the blind alleys than of entrances into them has any real value as to constructive learning, since learning is a changing process? May not the probability laws merely indicate why the animal gains entrance to the food box or goal and not account for the fact that in successive trials the errors gradually become eliminated until finally no errors are made? Let us go back to the

results obtained in the detailed empirical study of the learning (49), and see what light is thrown on the question. It will be necessary first to explain briefly the method of this part of the investigation. All choices in the first three trials of the 17 rats thus studied in detail were tabulated in a form to indicate both forward and backward movements in the maze, including all entrances to blind alleys, in the exact order of their occurrence. Every 'choice' except the first in any part of the maze was marked *F* if consistent with expectations based on frequency, that is, if the animal had made that choice more often than the alternative one, and *f* if contrary to such expectations; *R* if consistent with expectations based on recency factors (more recent choice of that alternative), and *r* if contrary to such expectations; *B* if in agreement with both frequency and recency expectations, and *b* if contrary to both. Whenever frequency factors were balanced for the two alternatives offered at any point and the response was contrary to recency expectations, for example, the "choice" would simply be marked *r*.

When all the tabulations were completed and studied it was clear that the animals had largely disregarded the association laws which philosophers and psychologists had built up through the ages since the time of the early Greek scholars. One could hardly have predicted at any point what a given choice would be. Moreover, it was found that in the early stages of the learning, reactions contrary to the expectations based on frequency and recency factors were more numerous than reactions in accordance with them. There were clear tendencies here and there for stereotyping processes to take place: rats would occasionally run from one blind alley to another, back to the first again, and so on. But where real learning took place, the animal ventured forward into new territory contrary to these association laws. Intensity factors were presumably controlled. It was concluded that "the *influence for learning* of recency and frequency in the early trials (where such influence would be most effective) is surprisingly small."[b] In many cases, as has been pointed out, the

[b] Thorndike (76) effectively criticized Stevenson Smith's view (63), that frequency alone may account for constructive learning since each trial must always end with the

influence of these factors is *against* learning, other factors having to throw the responses out of frequency-recency channels. These other factors are in all probability *visceral; they are larger bodily reactions away from monotonous* repetitions which are unprofitable to the entire organism" (**49**, p. 353). A little earlier in the same article these factors which "develop to throw the animal out of this routine" were thought to be "either changing external circumstances or . . . varying intra-organic factors" (p. 347).

The answer to our earlier question, then, as to whether the greater probability of runs past than into the blind alleys really has anything more to do with learning than merely to help the subject each time to get to the goal-end of the maze, is that because of the simplifications effected at the goal-end, the organism more readily reacts to the situation on the basis of consistency at that point than at any other, forcing the neural adjustments which are necessary for learning and for omission of entrance to the final blind alley; and that when this has been learned the simplification will next be found at the point of encountering the blind just previous to it, and so on, back to the first blind alley. Frequency and recency factors seem therefore to operate only indirectly in blind-alley elimination. They are not selective factors. On the other hand it is evident that holdover effects of stimulus and response processes may become very effective in eliminating blind alley entrances next to the goal.

These conclusions from early results were arrived at in 1917. Since that time the devising and use of the rational learning problem (**51**) and of other ingenuity problems like disc transfer (**58**, p. 31), which were practically insoluble on the basis of mere frequency of stimulus and of response, have strengthened the early hypothesis of "completeness of response;" and researches by Lashley (**35**), Köhler (**31**), Tolman

successful (goal attaining) act. Smith held that this act must occur more frequently than any other (irrelevant) act, because no other act necessarily had to occur in any particular trial. Thorndike pointed out that on strict frequency principles the first irrelevant act that appeared would simply be endlessly repeated. He, however, continued himself to stress frequency factors very much, in connection with his "law of effect," until 1927 (**77**).

(72), Norman Maier (36), and many others, even including Kuo (32, pp. 11 *ff.*), seem also to count heavily against the frequency theory in any form. It should be added that Thorndike, who seems not to have been aware of these earlier experiments which have been traced, but only of the statement of the hypothesis, if one may judge by his references and statements, has recently (77, 78, 79, 80), in a series of researches of much importance because of his influence on educational literature, converted himself and others to the futility of frequency and recency factors in learning. It would not be difficult for me, and doubtless for many other psychologists, to accept his conclusions almost completely, as applied to the experiments and controls which he has used, if he would state his "law of effect" in other than affective terms, and would not assume the operation of specific hypothetical "satisfiers" and "annoyers" as the selective agents. On this particular point (the "satisfiers"), it seems to me that his experiments establish nothing at all, while it is well known and widely acknowledged that erroneous or inconsistent reactions come into conflict both among themselves in the organism and with various surrounding physical and social conditions.[7] The

[7] All of Thorndike's specific "satisfiers," like saying "Right" or rewarding the subject, simply supply (or constitute?) additional exteroceptive stimuli. That the punishment of human subjects by saying "Wrong" or depriving them of a few pennies from money assigned them at the beginning of the experiment should not be very (if at all) effective in learning whenever the situation inflicts no other inconsistencies by "wrong" choices, is not surprising. Muenzinger (40, 41) has found similar results for humans but not for animals. My own experiment (57) showed that human subjects learned the mental maze even when the situation was so arranged that both the negative effects of frequency and recency, on the one hand, and of painful electric shocks for "right" responses, on the other, operated against learning. Here, certainly, consistency factors predominated over considerable obstacles. Dodson (15), with approximately "optimum" stimulus conditions, found that rats learned more readily in a discrimination problem with electric punishment for wrong choices than food rewards for right choices. Thorndike's chicks (78) made somewhat different responses to analogous, but not quite similar, situations. Watson's delayed feeding of rats (92) yielded results probably contrary to the view of satisfiers, but more recent experiments have cast doubt on those preliminary results, and the experiment should be repeated. For Thorndike, as for David Hartley (21), "pleasure and pain" seem to have some innate guiding qualities. Hartley's statement in terms of the thought of 1749 follows: "But it appears [from his own statements previous to this sentence] . . . that God has so formed the world, and perhaps (with reverence be it spoken) was obliged by His moral perfections so to form it, as that virtue must have amiable and pleasing ideas ffixaed to it; vice, odious ones" (p. 504).

nature of these conflicts and their effects toward the gradual or even rather sudden elimination of maladjusted responses in new situations, certainly needs careful study by all methods possible before we conclude either that feeling tone or specific "satisfiers" and "annoyers" are in any degree responsible for such changes as constructive learning. Most of the criticisms by various psychologists of Professor Thorndike's conclusions seem to aim at this point (*e.g.*, **9**, **4**, **22**, **43**, **72**, pp. 343 *ff.*; cf. Thorndike, **81**, **82**, **83**).

Seventeen years ago the assumption of hold-over effects of stimuli and responses was rather new. Today numerous experiments on higher types of animals make such effects a practical certainty. Not only do stimulus effects persist for short periods after cessation of the stimulus (**72**, ch. 8) but even slight responses of striped muscles result in volleys of centripetal proprioceptive impulses (**44**, p. 98) which again find their way out into other responding organs, producing continuous tendencies to certain lines of activity. These persisting stimulus-effects and motor tendencies doubtless have important results on the afferent impulses from more recent stimulation. Dodge (**14**) has recently emphasized the presence of numerous sorts of anticipatory reactions, most of which certainly depend on such continuations of organismic activities for appreciable periods after the cessation of particular stimuli.

The method of progressive elimination of blind alley entrances by less and less penetration in successive trials has now been recognized by different experimenters, and Hull (**27**) has attempted explanations of such elimination on the basis of his "habit-family hierarchies," which may probably represent activities surviving certain rather specific stimuli that have ceased; but it must be noted that they are as yet hypothetical and it is doubtful that they can be conditioned reflexes. Something more than mere contiguity association may be present in such gradual eliminations by lesser and lesser degrees of entrance into blind alleys. More data from carefully planned experiments are desirable before conclusions can be safely drawn.

The question as to the actual order of elimination of blind-alley entrances has received more attention; for example, by Hubbert and Lashley (**23**, **24**), Carr (**8**), Warden and Cummings (**84**, **86**), and Spence and Shipley (**65**). Several minor factors have proved to be influential. We may name particularly early development of forward-going tendencies (**48**, **11**), general orientation toward the food or goal (**24**, **10**, **97**, **99**, **72**, ch. 8), the different directions in which the blind alleys lead with respect to the goal (**65**, **72**), and such special factors as particular maze-patterns used. Spence (**64**) has recently analyzed the data from several investigations, and concludes that, despite the influence of such diverse minor factors and of the peculiarities of the various mazes used, a general tendency to backward elimination of blind alleys is evident in maze learning. This general tendency toward backward elimination of errors is regarded by Munn (**42**, p. 338) as one of the strongest bits of evidence for Hull's goal gradient hypothesis. In 1931 Hull (**25**) attempted to show by neural diagrams that "goal attraction and directing ideas" are conceivable as habit phenomena. He did not mean by these terms any mysterious pull or other force exerted by the goal or by "ideas," but his use of these terms probably indicated reference to a semi-popular view not unlike the "attraction of attention" or the "direction by meanings" in the older psychology.[8] Tolman (**73**) in a very recent report also finds evidence for the conclusion that "There seems to be something in the nearness to the goal (that is, spatial or temporal nearness, or both) which favors more rapid learning in discrimination boxes as well as in mazes" (p. 152). However these phenomena are interpreted, there is, of course, no actual pull by goal or food. The nerve impulses which stimulate the muscles for such locomotor activities toward the goal, involve energy supplied by the fibers

[8] The text is slightly changed here from that given in the address, which wrongly attributed to Dr. Hull himself the view of "attraction" by the food. Hull has worked consistently for years endeavoring to substitute definite neural bases for anything approaching mystic forces in behavior. His schematization of the "habit family hierarchies" (**27**) easily lends itself, in my opinion, to a view of learning based more on consistency factors than on repetition. See particularly his neural explanation of the progressive elimination of entrances to long blind alleys.

themselves, so there can be neither a pushing nor a pulling. This will be more obvious if other motives than food are utilized in maze experiments with this special point in view. Because of the importance of motivation problems, as shown especially by the work of Warden (**85**) and Stone (**66**), experiments on backward elimination of errors in mazes by non-feeding methods would seem to be of considerable aid in determining, first, the facts about orders of elimination of blind alleys under different circumstances, and, second, the merits of different hypotheses as to the real bases of such elimination. These are specific problems, easily investigated, and of a nature to throw considerable light on details of learning processes in general.[9]

In 1926 Washburn (**89**) suggested the conception of a goal gradient based on the supposition "that the first stimulus persists while the second one acts, so that the two are virtually simultaneous . . . that the readiness of the last movement in the series is transferred back to the movement just preceding it, the readiness of this movement back to its immediate predecessor, and so forth" (p. 332). "The beginning as well as the end of the maze running process is associated with the drive because *a drive is not a momentary stimulus like a sound or an electric shock, but a persistent physiological state that is present through the whole running.* Much of the mystery of the backward direction [of elimination of blind alleys in learning] disappears when we remember this fact," (p. 333). She recurs to the association laws and says: "The movements nearest the end of the series have a greater readiness due to recency of performance. Further, the drive itself may be supposed to be strongest just before it is put an end to by the redressing of the physiological balance" (p. 335). Endeavoring to represent these association processes more graphically and concretely, Hull (**26, 27**) has worked out in commendable detail various schematic neural diagrams based in part on Washburn's goal gradient idea. His view is "that the goal reaction gets con-

[9] Tolman (**72**, ch. 7) summarizes experiments by De Camp (**12**), Gengerelli, and Yoshioka (**98**) all of which show that the rat always chooses finally the shorter of two routes to the food. He also cites an experiment by Sams and himself that indicates a similar advantage for final choice of temporally shorter over temporally longer paths.

ditioned the most strongly to the stimuli preceding it, and that the other reactions of the behavior sequence get conditioned to their stimuli progressively weaker as they are more remote (in time or space) from the goal reaction" (p. 26). His schematic neural diagrams are designed to indicate the possible physiological mechanism of such a conditioning process. It will be noted that both Hull and Washburn assume stronger association progressively from the entrance of the maze to the goal end, attributing the accelerating factor to the persistent physiological state or food drive constantly operating through the entire run of the maze. It is not clear how these accelerating factors work, despite the reference to weaker and weaker backward association, if odor and specific place association factors be eliminated. Will the same phenomena be present when punishment is used as motive? Hull's recent demonstration (28) of accelerating speed by the rat in a straight runway as food is approached, is a beginning experimental attack on a rather crucial problem. But it is well known that speed of activity in various learning processes increases with learning (*cf.* 56), and this *general* increase may easily take on the appearance of greater increase *at the goal end*, simply because the animal will in each run have had more practice on the whole when it passes through the goal end of the maze than at any earlier part. The explanation of backward elimination already given in this paper accounts for the increased speed at the goal end of the maze, as being due to the greater learning coefficients or the more simple conditions for learning at that end than at the other. No experiment is very easily made crucial in deciding between the two views on this point. On one point, however, there is a clear difference between expectations based on Hull's hypothesis and those based on the "completeness of response" hypothesis. The former view is favored if more ready elimination of long blind alleys occurs; the latter, if short blind alleys are eliminated the more readily. A crucial test on a large number of animals, irrelevant factors being rigidly controlled, should yield valuable data. At present, data favor the latter hypothesis.[10]

[10] See discussions in references (93) and (54).

Finally, regarding hypotheses which attribute learning to effects of pleasure or displeasure, my own view is that they are not subject to experimental verification; that their alleged consistency with certain observed results, particularly as derived from experiments which leave the subject a sort of passive role in mere associative learning, is a misinterpretation based not only on general vagueness but especially on the inclusion of what statisticians call "spurious factors" in certain too high correlations.

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LIMITING FACTORS IN THE EFFECT OF THE REWARD UPON THE DISTRIBUTION OF ERRORS IN MAZES

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The role of the reward in determining the behavior of rats in mazes has been given considerable theoretical and experimental consideration in recent years. One of the reward factors that has been especially studied is the effect of the spatial position of the reward upon the orientation behavior of animals in mazes.

Hubbert and Lashley (**16**) observed that rats ran with their heads close to the inner walls of a Watson circular maze. This behavior was interpreted as orientation to the food position in the center of the maze. It should be noted, however, that this behavior can also be attributed to the fact that following the inner wall is the shortest path to the goal.

Lashley (**18**) reports that when twenty rats which had learned a maze were blocked at the entrance and allowed to escape to the top of the maze, five of the rats took the most direct path to the food-box and exhibited practically no random exploration. Three reached the goal by random paths and the remainder dropped into the first alley and ran the maze in the usual manner. Lashley admits that the probability of this result occurring by chance cannot be computed but concludes that "The available evidence seems to justify the conclusion that the most important features of the maze habit are a generalization of direction from the specific turns of the maze."

Dashiell (**10**) has contributed a most extensive study of direction orientation. He concludes from the results obtained in an open alley maze that the rats are oriented to the food position. Many experiments on short cuts in complex mazes

[1] From the Psychological Laboratory of the University of California.

have seemed to indicate the possibility of general food orientation. Of these experiments, we might mention those of Tryon (**29**), Honzik (**14**), and Shepard (**23**). Various insight experiments have also seemed to indicate that the rats are capable of orientation to the food direction [Higginson (**13**), Helson (**12**), Maier (**20**), Hsiao (**15**), Tolman and Honzik (**26**)]. Valentine (**30**) has repeated Higginson's experiment and obtained negative results. Differences in drive and the presence and absence of reward have been investigated in relation to their effect upon the distribution of errors into food-pointing and non-food-pointing blinds [Tolman and Honzik (**27, 28**)]. The goal gradient theory is based upon the effect of the reward decreasing with increased temporal and spatial distances (**17, 24**). Space limitations forbid the citing of further studies. The reader is referred to a paper on differential errors in animal mazes (**7**) for a more complete summary.

It will be noted at once that all of the above experiments have been interpreted as due to the effect of the goal, and that this effect is interpreted as a generalized type of effect, *e.g.*, general food orientation, exit seeking, etc. Recent studies have convinced the writers that the results of many of the above experiments, if not all, can be interpreted as the result of more specific factors which are only indirectly associated with the reward, and that the *general* factors which have heretofore been postulated or 'proven' as the causes of the behavior contribute but little to the obtained behavior.

In order to justify this belief it will be necessary to consider in some detail several recent experiments and their relations to some of the experiments cited above. Buel and Ballachey (**6**) investigating the phenomenon of choice-point expectancy (**8, 9**), discovered that for the last correct choice direction of the maze to be effective in determining the choices in preceding units of a linear maze, the preceding units must be structurally similar to the last unit of the maze. If, for example, the last unit of the maze was a T-unit while the preceding units were Y-units or open spaces which allowed the rat to express a right or left preference, the choices in these latter units were unaffected by a forced turn, error choice, or free choice situation

in the last maze unit. But, if the last unit was a Y-unit structurally similar to a preceding Y-unit, the correct choice direction of the last unit was found to be effective in determining the choices in the preceding structurally similar unit. The maze in this latter case was so constructed that the food was centrally located with reference to the long axis of the maze. In the case where the last unit was a T-unit, the food was necessarily at one side of the long axis of the maze for the forced turn and error choice groups, and on both sides for the free choice groups. This experiment indicates that the food position *as such* is not effective in determining the choices of the rats in preceding units. The essential condition for the reward to be effective in determining the choices in this maze seems to be that the reward must be associated with some *specific situation which occurs elsewhere in the maze* (*e.g.*, another unit similar to the last maze unit).

In this maze the food was not closely adjacent to one side of the critical unit, but was at the end of the maze, and about three feet from the nearest preceding unit. If the food were closely adjacent to one side of the preceding units would food orientation be established in choice situations? An answer to this question is obtained from the experiments of Yoshioka (**31, 32**). This experimenter used a simple diamond form of maze so constructed that the diamond could be made an open space, or by placing a smaller diamond within the open space, a two-alley choice-situation. Food was placed by varying degrees to the right or left of the long axis of the maze. The food alley, and the starting alley were at opposite apices of the diamond. The food position was varied to the right or left of the long axis of the maze by intervals of 22.5°. When the food alley was at 45° with respect to the long axis of the maze the food was closely adjacent to one side of the diamond. When differential illumination was controlled in this experiment statistically significant percentages of paths in the open diamond, and choices in the two alley situation in the direction of the food were not obtained. When the diamond was an open space more paths were close to the food side of the diamond than when the diamond contained two alleys. How-

ever, none of the differences was found to be statistically significant. The largest critical ratio was 1.8. The maze used by Yoshioka is almost identical to that used by Buel and Ballachey (**6**) with the exception that in the latter experiment two units (an open diamond and a two-alley unit) were used. It is clear that displacement of the food with respect to the critical choice point has not been conclusively demonstrated as an effective variable.

The reward is seen to be operative in the experiment on choice-point expectancy (**6**) as directly associated with a particular kind of situation. In Yoshioka's experiment where such a possibility did not exist, as there were no preceding units resembling the last turn of the maze, the reward was not significantly effective, even when placed closely adjacent to the critical unit.

However, proximity (linear) can be shown to be effective upon choice-point expectancy. Buel (**9**) and Spragg (**25**) have presented evidence that the effect of the last correct turn direction upon preceding units increases as the goal is approached. Ballachey [2] using a form of linear maze described in a recent article (**1**) has found that when food is placed directly in line with the maze one and one half feet from the last unit the percentage of obtained expectancy choices to the total possible percentage of such choices in a 12 unit maze for the first 30 trials is 47.35 per cent. When food is placed eleven and one-half feet from the last unit, this percentage is only 31.23 per cent.[3] Thus, there is a difference of 16.12 per cent due to the proximity of the reward to the last unit.

Is it possible that this factor (choice-point expectancy) is operative in a complex maze? As has been pointed out (**6**, **7**) a complex maze probably does not present similar units. The sequence of spatial patterns formed by the units in the usual non-linear maze may serve to identify or differentiate each choice-point from the remainder so that few elements are present in the choice situations which are also present in the

[2] This result is an incidental finding in an experiment to be reported shortly.

[3] It is to be noted that Ballachey (**1**) has found that with long mazes (many units) the choice-point expectancy factor is less effective than in shorter mazes (fewer units).

reward-associated situation. But, if the maze were so constructed that the food-pointing units were also the units in which this factor could operate, it might seem from the error scores that there was a general food-orientation. Actually there might not be such an orientation.

Dashiell and Bayroff (**11**) have postulated two factors in the maze, the forward going tendency, and general direction orientation. But, Ballachey and Buel (**3**) found general direction orientation to be less effective in determining the choices of rats than the specific factor of centrifugal swing.

In order to determine whether or not food orientation is effective in determining the relative difficulty of blinds in a complex T-maze, Ballachey and Buel (**2**) made a correlational analysis of Tolman and Honzik's (**27, 28**) data upon the effect of differences in reward and degree of drive upon the maze performance of rats. It was found that differences in degree of drive or the absence or presence of reward (non-rewarded and rewarded groups) did not at all affect the order of relative difficulty of blinds. Thus, in this maze at least, food orientation is not an effective variable in determining the relative difficulty of blinds. Further, in this maze the corrected correlations between the error order of the first trial and the error order for trials 2–11 ranged from +.70 to +.83. It was found that the reliabilities of the first trial error order for all groups ranged from +.91 to +.97 and for trials 2–11 from +.97 to +.99. Thus, it is seen that the relative difficulty of blinds is due almost entirely to systematic factors even on the first trial. Furthermore, many of the factors present in later trials are present on the *first trial.*[4]

[4] Spence (**24**) has used Tolman and Honzik's data on the relative difficulty of blinds to test the validity of the goal gradient hypothesis and food orientation. "He obtained rank order correlations between the experimentally obtained order of difficulty of blinds and the theoretical order of difficulty to be expected if the goal gradient mechanism and goal orientation were the sole determiners of the relative difficulty of blinds." Presumably he used the data from the hungry rewarded groups. The correlations he obtained were +.90 and +.91. Ballachey and Buel (**2**) "computed the correlation for the hungry non-rewarded group. This correlation is +.92 and definitely proves that the goal orientation of Spence is not food orientation. The correlation between the theoretical order and the obtained order for the *first trial* computed by Ballachey and Buel (**2**) proved to be +.65 for the hungry non-rewarded group and

Let us now critically examine Dashiell's study of direction orientation (**10**). Dashiell used an open alley maze which allowed rats to take twenty equally economical paths to the goal. Any other path, of which there were many, involved excess distance in reaching the goal. Dashiell found that rats took variable routes ('equally economical') to the goal. He concluded from this that the rats were sensitive to the goal direction since they were capable of reaching the food box by variable routes. He also postulated an initial set at the entrance box which determined the initial direction of the route taken by the rat.

There are two studies which throw considerable light upon the actual mechanisms involved in the selection of these routes. The first of these is an experiment by Bledsoe, Blodgett, and McGarrity (**5**). These experimenters attempted to isolate the factor of distance from that of orientation. They used a circular maze made up of twelve concentric circles. "The path in one half of the circles was equal, and in one half was not. This enables the animals to respond in some of the circles in the direction away from food without involving the traversal of a greater distance. They were also allowed to respond in some of the circles in the direction of the shorter distance." They found that the choices in the direction of food in the equal alleys was only .18 sigma greater than would be expected by chance alone. The responses into the short alleys over the long alleys exceeded chance by 8.63 sigma irrespective of whether the alleys were food-pointing or non-food-pointing. This suggests that excess distance may be a more important factor than food orientation.

The second experiment is by Luh and Shen (**19**). These experimenters used an open alley maze of the same general structure as Dashiell's but which was more complicated in that it contained 252 equally economical paths to food. The subjects were 14 mice. After the mice had reached the criterion of three successive "errorless" runs (routes not involving

+.66 for the hungry rewarded group. These correlations indicate that a number of specific factors unrelated to the goal gradient and goal orientation (because they are present on the first trial) tend" to spuriously increase the correlations obtained by Spence.

excess distance) the correct runs out of the total number of subsequent runs was still very small. Only 2 of the 14 mice reached 50 per cent. "These figures indicate that taking one out of many 'equally economical' routes is quite a different problem from that of fixation of a definite route through a complicated maze. In the latter case after a strict criterion is once reached, most of the subsequent runs are more likely to be errorless than not." The average number of errorless runs per rat for each of seven successive 30 run periods was 7.3, 7.4, 12.1, 12.4, 13.8, 13.9, and 16.1, respectively. Two routes were found to be the most frequent for *all* mice. These routes were the only possible ones in which the animal could reach the reward chamber with only one turn. The average number of different routes taken was 28. 129 of the possible 252 correct routes were taken. The percentage of the frequency of the two most frequently taken routes to the total number of routes taken was 21.2 per cent for one route and 21.0 per cent for the other. Thus 42.2 per cent of the total number of correct routes taken were due to these two most frequently taken routes. The animal at the starting unit was found to go either to the right or to the left, depending upon position habits or chance, and then ran along a straight alley until "it reaches its end, makes a sharp turn and then takes another long stretch to the goal." The two routes were similar, one being to the right and the other to the left. "The chances were 1 to 1 for the animals to be oriented toward the right or left. . . . In a word, in this type of open alley maze the animal is oriented by chance at the first bifurcation and then as likely as not (42.2 per cent) it will keep the forward going direction as far as it can until *incidentally* it reaches the goal by the most economical route."

Besides these two stereotyped routes, 12 other routes which were considered to be slight modifications of the two most frequently taken routes were taken very frequently. The frequency of these 14 routes constituted 73 per cent of all the correct runs. Luh and Shen conclude that all these runs may be explained as due to initial orientation at the entrance box and to the forward going tendency.

They say: "This kind of open alley maze gives a very good opportunity for the free play of what Dashiell in another connection called the 'forward-going tendency' (11). Given initial bodily orientation (chance and position habits) and chance deviation from straight line runs, this forward going tendency will almost necessarily bring the animal" to the two most preferred routes. "There is no reason why this tendency alone should not be sufficient to account for all the non-stereotyped yet errorless runs. Dashiell rightly concluded that the forward going tendency is not the same sort of determining factor as direction orientation. He further insisted upon the fact that direction orientation could not be invoked to explain away the data gathered from his recent experiments which gave clear evidence for the forward going tendency. *But he never considered the possibility of reducing direction orientation to this forward going tendency.* The bare fact that there are non-stereotyped runs and that their number is much larger than chance would warrant for does not at all indicate the working of direction orientation." A decrease in the number of stereotyped runs to the total number of errorless runs was observed. This led Luh and Shen to conclude tentatively, that "there seems to be another 'determining tendency' which controls the bodily turns of the animals in a more flexible and general way. It enables the animal to take variable routes toward the same goal." The increase in variability is not marked and the authors do not conclude that this determining tendency is a general orientation.

It should be noted that these conclusions are what Schneirla (22) predicted from the factor of centrifugal swing. The forward going tendency has been modified by Ballachey and Krechevsky (4), Schneirla (21) and Ballachey and Buel (3), who have pointed out that its operation is not due to a tendency but to actual bodily displacement. Whether the behavior in the open alley maze is really forward-going tendency or centrifugal swing, it is apparent that direction orientation has not been clearly demonstrated, and that a specific factor may account for the results.

In our review we have considered both simple spatial

situations and complex ones, and in none of the situations has it been clearly demonstrated that the factor of general orientation accounted for the results. In all of the situations it has been possible to account for the results as due to some specific factor which is only indirectly associated with the goal, and in none, with the possible exception of Yoshioka's experiment, with the spatial *direction* of the goal.

It is possible that in the ordinary maze which usually has the goal and exit so placed that the maze roughly forms three sides of a square, the turns in direction and the direction in which the blinds point are too often consistent with specific factors (*e.g.*, centrifugal swing, choice-point expectancy, etc.), and the direction of food. That more errors are made into food-pointing blinds than non-food-pointing blinds may be an accident of maze design, rather than the result of some general factor of orientation.

The experiments we have cited are but a small sample of spatial situations. We do not doubt that orientation is possible for rats, but we are inclined to the notion that it is far less important in determining behavior than was heretofore thought. Different species must certainly have varying degrees of ability to generalize direction. Köhler's experiments on apes seem to indicate that this animal is capable of integrating complex spatial situations. Tolman and Honzik (**26**) in their investigations of insight in rats found that it was very difficult to set up a situation in which the rats could integrate three paths to food in such a way that when the common path to the goal from the two most economical paths was blocked, the rat would take the longer route to food (non-preferred path).

It would seem from the above considerations that a revaluation of so-called orientation experiments should be made. The goal may be associated with specific situations in which an error in judgment on the part of the rat may lead to choices in the maze that can be interpreted as goal orientation. Other factors which are not at all closely associated with the goal may cause behavior similar to that expected as a result of goal orientation, though this may be due to an accident of

maze pattern and the operation of factors unrelated to the goal direction.

In order to obtain an independent validation of the results found for a T-maze (**2**) which seemed to indicate that food orientation had no effect in determining the relative difficulty of blinds, the following correlational analysis was made of the distribution of errors in a twelve unit maze of different design and pattern. In this maze each unit is preceded by a forced turn (right angled elbow). The units are identical throughout the maze. The structure of these units may be found in a recent article (**1**). The maze pattern is complex spatially, the goal direction being counter clockwise. This maze is diagrammed in a forthcoming article.[5] In this maze, because of the forced turns preceding each unit a known factor (centrifugal swing) determines the locus of errors in the maze.

Two groups were employed. By changing the sequence of true paths turns (blocking one alley or the other of each maze unit) centrifugal swing forced the rats in group 1 into the blinds in units 3, 5, 7, 9, and 11. For group 2, centrifugal swing acted to cause entrances into the culs de sac in units 2, 4, 5, 7, and 10. The general maze pattern was the same for both groups.

The weight of centrifugal swing as a factor determining choices is indicated by the percentage of errors in those units in which this specific factor causes error choices. These percentages were obtained by expressing the total number of centrifugal swing errors as a percentage of the total number of errors in the maze (unit 1 was excluded). The percentage, for the first trial, is, for group 1, 85.4 per cent; and for group 2, 88.2 per cent. Only 41.7 per cent of the errors would be expected to occur by chance in the 5 centrifugal swing blinds. Thus, we see that centrifugal swing has a very heavy weight in this maze on the first trial, although other factors may be operative in determining the difficulty of the blinds.

[5] The description of the experiments from which these maze data have been taken are at present on file in the University of California Library as part of a Ph.D. dissertation entitled "An analysis of 'cognitive' factors in maze transfer in the white rat," by E. L. Ballachey.

The correlations between the relative difficulty of blinds for the first trial and for trials 2–17 are given in Table I. The

TABLE I

THE CORRELATIONS BETWEEN THE ERROR DISTRIBUTION OF TRIAL 1 AND TRIALS 2–17 *

	Correlation between Trial 1 and Trials 2–17		Reliability Coefficients	
	Uncorrected	Corrected	Trial 1	Trials 2–17
Group 1.....	.87	.89	.96	.98
Group 2.....	.83	.85	.97	.97

* The probable errors of the correlations reported here are meaningless and have not been computed. The assumption of the probable error technique is that the sample dealt with is a *random* selection from the universe. The 12 blinds we are here considering are a unique sample and are not a *random selection*.

uncorrected correlation for group 1 is + .87, corrected, + .89.[6] The raw r for group 2 is + .83, corrected, + .85. These correlations are only slightly higher than the correlations obtained in a previous study by Ballachey and Buel (2). The reliability coefficients indicate that systematic factors almost entirely determine the distribution of errors for both trial 1 and for the later trials. Again, this is in accord with the results of the previous study (2).

[6] The corrected r's in Table 1 were computed according to the formula derived by Dr. R. C. Tryon. The usual formula for the correction for attenuation is not applicable to these data because the *same* rats were employed in getting the two measures from which the *raw* inter-correlations were calculated, whereas different rats were in the two measures from which the reliability coefficients were calculated. The reliability of a group for the first trial was obtained by halving the sample (x_1, x_2) and (y_1, y_2). In order that the same test items (rats) should not be present in the inter-correlations and spuriously raise the correlation, x_1 was correlated with y_2 and x_2 with y_1, where x_1 and y_1 are the error scores of the same rats for trial 1 and trials 2–17, respectively. The formula is:

$$r_{\infty} = \frac{\frac{r_{x_1y_2} + r_{x_2y_1}}{2}}{\sqrt{r_{x_1x_2} \quad r_{y_1y_2}}}.$$

The reliability coefficients in the denominator are the reliabilities of one half the sample, *i.e.*, the S-B formula has not been used to obtain the reliability of the entire sample. The reliabilities of the halves are used because the correlations in the numerator are the coefficients between halves.

In this maze, in contrast to the maze used in the previous study, the most heavily weighted factor (centrifugal swing) is known. Despite the operation of this heavily weighted factor, the correlations between the first trial and later trials are not unity. Is this due to the fact that food orientation is developed in later trials and affects differentially the difficulty of the various blinds in the maze? To answer this question we have computed the percentage of errors into the food-pointing blinds for trial 1 and for trials 2–17. In order to give food-orientation a maximum opportunity to operate, the following arbitrary definition of food-pointing blinds was employed: every blind which pointed down (toward the goal) or pointed to the left (in the goal-direction) was considered as food-pointing with the exception that all blinds only one 'maze-unit distance' to the right or left of the food-position were not classed as food-pointing. In such units, the deviation was not thought to be great enough to produce food-pointing responses. For trial 1, the percentage of food-pointing errors to the total number of errors is 33.5 per cent for group 1 and 49.7 per cent for group 2. The percentages for trials 2–17 are, for group 1, 44.2 per cent and for group 2, 48.6 per cent. It will be noted that the percentage of responses into food-pointing blinds increases for group 1. However, the percentage of food-pointing errors for trials 2–17 is *not* significantly different from chance (41.7 per cent). The percentage of entrances into food-pointing blinds for group 2 is slightly higher on the first trial than for trials 2–17. This indicates that food-pointingness is not effective in this maze, and cannot account for the fact that the correlations between the distribution of errors for trial 1 and trials 2–17 are not unity. These data are given in Table II.

The difference in percentages between group 1 and group 2 is an interesting example of the accidental effect of maze design upon the percentage of entrances into food-pointing and non-food-pointing blinds. In group 1, only one unit in which centrifugal swing operates, also points in the food-direction (food-pointing blind). In group 2, two centrifugal swing units are also food-pointing. If, as is the case in the

TABLE II

THE AVERGAE PERCENTAGE OF ENTRANCES INTO FOOD-POINTING BLINDS *

	Group 1	Group 2
N.	30	33
Trial 1:		
Av. per cent.	33.5	49.7
Chance.	41.7	41.7
σ 41.7 per cent.	3.79	3.94
41.7 per cent + 3 σ	53.04	52.49
Trials 2-17:		
Av. per cent.	44.2	48.6
Chance.	41.7	41.7
σ 41.7 per cent.	1.57	1.66
41.7 per cent + 3σ	46.38	46.65

* σ 41.7 per cent $= \sqrt{\frac{p\ q}{n}}$; p = 41.67 per cent, q = 58.33 per cent, n = total number of choices.

usual maze, the specific factors were unknown, the distribution of errors might be taken as evidence for general food orientation. In the T-maze used by Tolman and Honzik (**27, 28**) other factors than centrifugal swing are operative in the first trial. This is indicated by a preliminary analysis of this maze.

Buel (**7**) has pointed out that there are many factors in the maze which may account for differential errors. Actually 94 factors which may account for errors have been found by this writer. He says: "*A thorough analytical examination of the maze demands that not only must the factors themselves be identified, and the relationships existing among them be known, but also the degree to which they contribute to variations in behavior.*" The correlations between the error distributions for the first trial and later trials indicate that many of the factors which determine errors in the first trial are the same as those determining errors on later trials. If the number of factors which operate in this way is large, as has been indicated, then the operation of a considerable number of them must be independent of the reward or maze experience, since they occur on the first trial. Heretofore it has been thought that the reward and maze experience contributed markedly to the distribution of errors.

The conclusion that the role of the reward is relatively ineffective as a general factor determining the behavior of

animals in either complex or simple spatial situations is indicated by the above analysis. It is suggested that the role of specific factors should be more thoroughly investigated, since such factors have been found to account for behavior heretofore attributed to general factors related to the reward.

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THE ORGANISM AND THE CAUSAL TEXTURE OF THE ENVIRONMENT[1]

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Having found that our previous separate investigations had led us quite independently of one another to a common point of view as to the general nature of psychology, we decided upon this joint article.

I

Each of us has come to envisage psychology as primarily concerned with the methods of response of the organism to two characteristic features of the environment. The first of these features lies in the fact that the environment is a *causal texture* (*Kausalgefüge*)[2] in which different events are regularly dependent upon each other. And because of the presence of such *causal couplings* (*Kausalkoppelungen*), actually existing in their environments, organisms come to accept one event as a *local representative* (*Stellvertreter*) for another event. It is by the use of such acceptances or assertions of local representatives that organisms come to steer their ways through that complex network of events, stimuli and happenings, which surrounds them. By means of such *local representation* (*Stellvertretung*) the organism comes to operate in the presence of the local representative in a manner more or less appropriate to the

[1] This article was written during a relatively long stay of the one author, Tolman, in Vienna. A somewhat different version under the title "*Das Lebewesen im Kausalgefüge seiner Umgebung*" will, it is hoped, appear later in German. The authors have sought throughout to bring their two sets of terminologies into correspondence. The parallel German terms are presented here in parentheses.

[2] For the term "texture" as well as for advice on various other English terms we wish to express special indebtedness to Professor S. C. Pepper. (See also 29.)

fact of a more distant object or situation, i.e. the *entity represented* (*das Vertretene*).[3]

The second feature of the environment to which the organism also adjusts is the fact that such causal connections are probably always to some degree *equivocal* (*mehrdeutig*). Types of local representatives are, that is, not connected in simple one-one, *univocal* (*eindeutig*) fashion, with the types of entities represented. Any one type of local representative is found to be causally connected with differing frequencies with more than one kind of entity represented and vice-versa. And it is indeed, we would assert, this very *equivocality* (*Mehrdeutigkeit*) in the causal "representation"-strands in the environment which lend to the psychological activities of organisms many of their most outstanding characteristics.

It appears also that, whereas the one of us, Tolman (**33**), was led to emphasize these two facts of *local representation* and of *equivocality* (*Mehrdeutigkeit*) by a study of the relations of *means-objects* (*Mittelgegenstände*) to *ends* (*Zielgegenstände*) in the learning activities of rats, the other, Brunswik (**2**) was led to emphasize these same concepts as a result of an examination of the relations of *stimulus-cues* or *signs* (*Reize als Anzeichen*) to *Gegenstände*[4] as a result of a study of the relations in-

[3] The first modern psychologist to suggest the universal importance of this principle of "representation"—the scholastic "*aliquid stat pro aliquo*"—for *all* psychological phenomena was Karl Bühler (**7**). He has emphasized in particular the "sign" function of local representatives in their different forms, *i.e.* as "*signals*" for action and as "*Anzeichen*" in reception. He has made an especially important analysis of the sign function of "*symbols*" in his psychology of speech (**8**).

For another modern emphasis on the sign-function in perception and thought see Ogden and Richards (**26**).

[4] The word "*Gegenstand*" has been employed by Brunswik (**2**) and will herein be further employed to designate, not complete environmental objects or bodies in their concrete totalities, but single object-characters abstracted from such total bodies. Such abstracted characters are conceived and defined in completely objective fashion. They are discovered and identified by processes of measurement and computation as these latter are carried out either by physics or by the more ordinary procedures of practical life. And it appears thus that in any single total *behavior-object* (*Korper*, "*Ding*") there intersect numerous simple *Gegenstände*, such, for example, as: size, form, reflection-coefficient for light waves (*i.e.* physical "color"), hardness, weight, density, volume, chemical characteristics, etc. All these properties might at different occasions (in different life-contexts) become in different manner biologically important, or, to use the concept of Karl Bühler, become in different manner "*abstractively relevant.*" From this standpoint the properties of a means-object, characterized pre-

volved in the "Konstanz"-phenomenon in human perception.[5]

We observe animals making and using tools, entering paths, ingesting food, avoiding dangerous objects, and the like. But in each such case the tools, the paths, the foods, the dangerous objects are behaved to only because of their role as means-objects. They are behaved to, that is, in their roles as the most probable "local representatives" whereby to reach or avoid such and such more ultimate, "represented" positive or negative, goals. For it is the reaching or avoiding of these more distant represented goals which are of final importance to the organism. And further, we also observe these same animals, responding selectively, (and perhaps in the ordinary case relatively correctly), to immediate entities (*e.g.*, the detailed structure of light-wave bundles, and the like) in their turn, as the most probable local representatives, *i.e.*, cues, for such tools, paths, foods, dangerous objects, etc. And here, also, it is the character of these more distant "represented" objects which have the greater determining significance for the organism. Light-wave bundles, and the like, are to be correctly selected as the most probable local representatives, *i.e.*, as cues, for such and such object-characters, just as the latter must themselves be correctly selected as the best local representatives (*i.e.*, as means-objects) for the finally to-be-reached or to-be-avoided goals. Without the ability to rely on these

viously by Tolman as *discriminanda, manipulanda, utilitanda*, are to be conceived as groups of *Gegenstände*, which are different with respect to their abstract relevancy for the organism. (Cf. **26**, Chapter III.)

Further, because of its generality and abstractness this word *Gegenstand* can be used not only for the properties of *means*-objects but also for the *cue*-properties of peripheral stimulation-processes (*e.g.*, intensity, form, or size of the projection of an object at the retina, the visual angle, etc.) as well as for such internal events or states as goal-satiation, and the like,—in short for everything, which can be defined in terms of physics (or geometry, etc.) and which is therefore capable of objective measurement.

[5] It should also be noted that of the two of us it was primarily Brunswik (**2**, pp. 29 *f.*) who previously emphasized the importance of the feature of equivocality (*Mehrdeutigkeit*) in the environmental causal couplings. This sort of *Mehrdeutigkeit* is, of course, not to be confused with the possibility of a subjective "*Gestaltmehrdeutigkeit*," *i.e.*, with the fact—first emphasized by Benussi—that one and the same stimulus-configuration may on different occasions be responded to by quite different perceptual impressions.

two successive types of local representation no higher forms of organism could have developed and successfully survived.

Finally, it is to be pointed out that because of the equivocality (*Mehrdeutigkeit*) that always to some degree obtains in both such steps, *i.e.*, in the relations between cues and means-Gegenstände and in those between the latter and goals, the organism is led in both instances to the assertion of "hypotheses." That is, whether in the process of selecting the correct means-object (Gegenstand-complex) to reach a given goal or in that of selecting the correct cue-Gegenstände for perceptually identifying a means-Gegenstand, the organism is forced to venture an hypothesis.[6] We would here introduce, that is, the term hypothesis as not only appropriate and inevitable for the case of discursive thought, for which it was originally coined, but also for such simpler lower-order situations as are here involved in immediate perception and in the simpler sorts of means-end activities. Thus, whether the case be that of a father, who, as a result of his reading and previous experimentation, ventures a discursive verbalized hypothesis to the effect that the conditioned reflex is the fundamental principle of all learning and proceeds thereupon to try to make his children love Latin as a substitute-stimulus for chocolates; or whether it be that of a rat, who, from having been run through a discrimination-box and having found the lighted alley always open, tends "hypothetically" to choose this alley continuously for some time afterwards (whether or not the latter then still leads to food); or whether, finally, it be that of a monkey or a human being who, upon having projected upon his retina the characteristically fuzzy grading-off edge of a dark area, sees this dark area as a shadow and not as a separate spot with a blacker surface-quality; the essentials are the same. In each such case the organism behaves "as though." That is, he ventures an hypothesis. He may be right; but he may also be wrong. A fuzzy edge in the given case may surround not a shadow, but a spot with separate surface-color.[7]

[6] This use of the term "hypothesis" in a purely *objective* sense was first made by Krechevsky (23) and has since also received the approval of Claparède (9). The objective ear-mark for such hypotheses lies in the appearance of systematic rather than chance distributions of behavior.

[7] For all the various possibilities in this sort of situation see Kardos (21).

The lighted door in the given instance may lead, not to food, but to electric shock. The giving of Latin before chocolate may result not in the child's coming to love Latin, but merely in an unpleasant propensity to secrete saliva while studying Latin.

An hypothesis "asserts" that a given "*a*" is the local representative of a given "*b*." But the connections between types of local representative and types of entitites represented are, as we have said, practically never "one-one." Any given type of "*a*" is probably always capable of being in varying degrees the representative of a number of different types of "*b*." And any given type of "*b*" is probably always capable of being represented with different degrees of frequency by each of a number of different types of "*a*." Any particular hypothesis therefore that a given "*a*" on a given occasion means a given type of "*b*" will have only a certain probability of being valid. The degree to which such an hypothesis will tend to be valid or merely superficial and hasty will vary with the degree to which, "normally," the given type of cue-Gegenstand does tend to be coupled in "relatively one-one" *i.e.*, *univocal*, (*eindeutig*) fashion with the given type of to-be-perceived (*intendiert*) means-Gegenstand or upon the degree to which the latter does tend to be normally coupled in "relatively" one-one fashion with the given type of goal-Gegenstand.

As we have indicated, it is to be one of the main tasks of this essay to indicate the further significance for the psychologies of perception and of means-end action of just such lacks of complete *univocality*.

But first we wish to present a single simplified scheme for combining perception and means-end action into one picture (on the oversimple assumption of univocality).

II

Figure 1 is a diagram to represent the combined perceptual and means-end activities of an organism. This diagram involves the simple but incorrect assumption (to be corrected by later diagrams) of solely univocal, one-one (*eindeutig*) cor-

respondences between goals and means-objects and between the latter and cues. In this figure the area above the v-shaped continuous line (*i.e.* a "v" with a curved bottom) represents the environment, whereas the area below this line represents the organism. Let us suppose that *b* indicates a

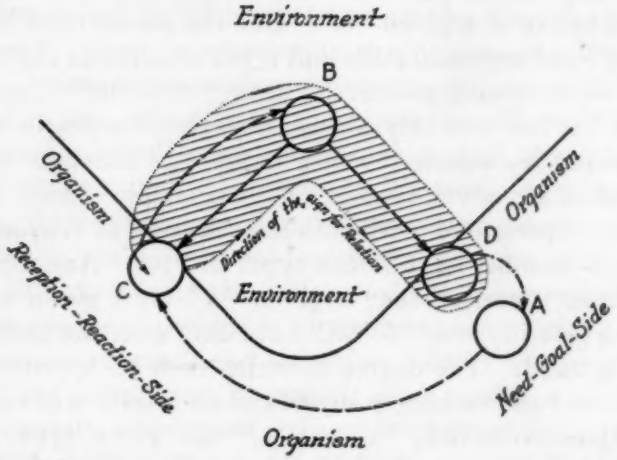

FIG. 1. Organism and behavior object—with assumption of univocal, one-one, couplings between cues, means-objects and goals. The cognitive lasso principle.

behavior-object (*Hantierbarerkörper*) *i.e.* a possible means-object in the visual field, *e.g.* food, which, as such, has the characteristic that it is an appropriate possible cause (with the cooperation of the organism) for resultant satiation, *d*. Independently of the organism this object *b* radiates causal trains, *e.g.* light-waves, in many directions. And part of these lead (continuous arrow *bc*) to the sensory surface of the organism. Let us assume, further, that other objects of the same variety as *b* have previously sent visual influences of this same sort to the organism. And let us also suppose that previous trial and error activities on the part of the organism have demonstrated the *behavior-manipulability* (*Hantierungstauglichkeit*) of these *b* sorts of object. And, finally, let us suppose that the outcome of such behavior-manipulations led in each past case to satiation *d*, *i.e.* let us suppose previous experience by the organism of the *utilitability* (*Erfolgstauglichkeit*) of things like

b. The total organized experience resulting from all these previous causal currents means the present readiness of a system of "hypotheses" concerning the various different actual or possible causal chains connected with *b*,—that is, as to the probable suitability of any new *b* as a means for reaching, *i.e.*, as a cause for resulting *d* and also as to the fact that the given peripheral stimulus at *c* probably results from (has probably been caused by) a *b* sort of object.

If, now, as a result of some internal activity, say hunger *a*, there comes an influence from the *need-goal-side* (*Bedarf-Erfolgseite*) of the organism to the *reception-reaction-side* of the latter (broken arrow *ac*), resulting in an opening of sense-organs and in the activation of this hypothesis-system, this latter together with the peripheral stimulus-configuration coming from *b* will lead to a reactional event *c*. In this reactional event *c* the peripheral stimulus has assumed the function of a *sign*, *cue*, (*Anzeichen*) indicating an actual *b* and "transitively" through *b*, a possible final *d*. In this event *c* the total past and present causal complex—indicated in the diagram as surrounded by the dotted loop—is anticipatively *lassoed* (*Lassoprinzip*). *C* thus has the character of a *sign-gestalt* (*Zeichengestalt*).[8] It appears, therefore, further, that if the situation be one of univocal relations or, that is, if it be a situation in which the anticipatory achievement of the lasso will be in all cases of this type correct, then it can be said that the means-object and also the goal have been by means of this lasso or sign-gestalt, *intentionally attained* (*intentional erreicht*) (Brunswik, 2).[9]

[8] Previously (33, 34) Tolman used the term "sign-gestalt-expectation" for the organic event and the term sign-gestalt for the objective environmental complex corresponding (in the case of correct behavior) to this organic event. Here, however, it seems simpler for the term sign-gestalt-expectation to be omitted and to use the term sign-gestalt for the organic event alone. The environmental entity or entities (with reference to which the organic event—the sign-gestalt—occurs) are, as we are emphasizing throughout this article, to be conceived and described as simply some area within a total environmental causal texture. Such an area will contain as its most essential feature strands of "local representation."

[9] It should be pointed out that one of the important features of the type of psychology here being argued for is that it demands and makes possible a characterization of the fundamental capacities of the organism in terms of the types of object and goal which the given organism is capable of thus "intentionally attaining." It is this

We note next that broken arrows indicate those causal chains in which the activity of the organism itself is necessary. Thus the broken arrow *cb* (issuing from the sign-gestalt *c*) is intended to depict the actual manipulation of *b*, grasping, eating, etc. And the outcome of this manipulation of *b* is indicated by the broken arrow *bd*. This latter action is to be conceived as resulting out of such manipulation, that is, as occurring without further independent activity on the part of the organism and it brings about the final goal situation *d*. Finally, after the attainment of *d* there will occur (after some interval of time) as a result of physiological processes (which need not concern us here) a new appearance in the organism of the need *a* (broken line *da*). And thereupon the whole circular process will once again be set into action.

III

Figure 1 presented the scheme of an organism in its environment for a very simple case—namely, that in which one step only is involved both on the left-hand and on the right-hand sides of the diagram. But organisms often meet situations involving a succession of cues or a succession of means-objects or both. Figure 2 is therefore now presented to show types of further extension of the diagram which are necessary for cases involving more than one step between cue and behavior-object or between the latter and the final goal.

The nature and meaning of Fig. 2 will be understood most easily if you apply it to a concrete example. Let us suppose that the organism in question is a child in his crib and that the object *b* is a piece of chocolate. We shall suppose further, however, that the latter is beyond the child's own reach.[10]

feature which Brunswik had in mind when he called his a "*Gegenstand-psychology*" ("*Psychologie vom Gegenstand her.*"), that is, a psychology from the standpoint of the organism's ability intentionally to attain Gegenstände. This type of an objective psychology is outlined theoretically in (2) and (3). An article in English concerning the main experimental results and the fundamental concepts is also in preparation.

For another somewhat related treatment of the interconnection of the organism with its environment see the "Umweltlehre" of Uexküll (36).

[10] If the child were able to reach the chocolate himself, the *adbc* part of Figure 2 would suffice. The whole situation would in fact reduce again to that represented in Figure 1.

He requires therefore some second object as a secondary means to the chocolate. And let us suppose further that there are in the room both good-willed and less-favorably willed individuals. The child can use the assistance to be provided by the good-will of one of these good-willed individuals. This good-will will serve, in short, as the second means-object b_1, suitable for achieving the first means-object (the chocolate, b). But

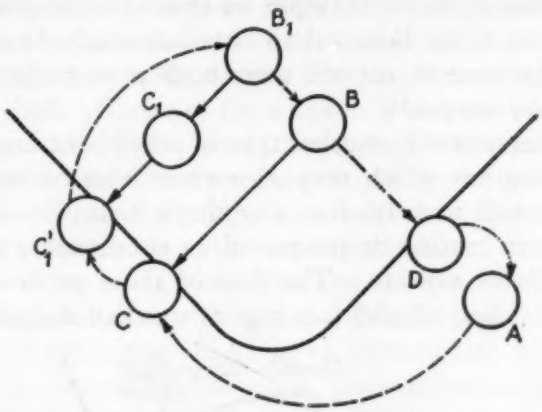

FIG. 2. Example of lengthened means-goal and cue-means chains.

this good-will b_1, this secondary means-object, lies shut-up within the psycho-neural make-up of the other individual. It can send no direct cues to the sense-organs of the child. The perception of it has to be mediated causally through some external characteristics in the other person's face. The facial expression of the other individual must, in short, serve as an intermediate cue c_1 between the final cues c'_1 on the child's retina and the ultimately to-be-perceived means-object b_1—the good-will (or the bad-will) of the other individual. Such an example thus presents a double step on both the reception-side and the means-side of the activity. The retinal effects on the child's eyes serve as local representatives, signs, for the facial expression of the individual. And this facial expression as a local representative serves in its turn as a sign (or sign-system) for the will (good or bad) of the other individual. Again, on the right-hand side of the diagram, the will of the other individual is a local representative of, and the means to,

the presence of the chocolate and then this chocolate is, in its turn, the local representative of and the means to (through perhaps some still further steps) final satiation.

It is evident that the general scheme of Fig. 2 could be extended indefinitely to allow for long trains of intervening means-objects or long trains of intervening cues, or both. Or again, it could easily be modified to allow for various special types of case such, for example, as that in which two means-objects have to be behaved to simultaneously—or in which one and the same object will serve both as secondary cue and as secondary means.[11]

Consider now still another type of possible extension of the original diagram which may also sometimes be needed. It must be noted, namely, that any single behavior-object such as *b* must in reality be conceived as subdivisible into three distinguishable aspects. The first of these parts or aspects (groups of Gegenstände) (see Fig. 3) we shall designate as the

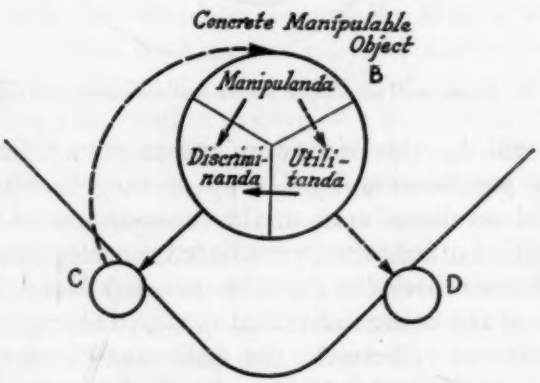

FIG. 3. Aspects within the single behavior-object.

discriminanda properties of such an object. These discriminanda would be such properties (Gegenstände) as the object's color, shape, size, etc., which are the relatively direct causes of the immediate sensory cues. They are the properties whereby the object is differentiated, discriminated from other objects. As the second part or aspect of a single be-

[11] An example for this latter would be paper currency which, at least in former times, served both as a cue for and a means to gold.

havior object we would designate its *manipulanda*-properties. The manipulanda of an object are, so-to-speak, its essential, behavioral core. They are the properties which make possible and support such and such actual behavioral manipulations. They are the object's grasp-ableness, pick-up-ableness, chew-ableness, sit-on-ableness, run-through-ableness, and the like. Finally, as the third aspect or part of a behavior-object we have what we shall designate as its *utilitanda* properties. The utilitanda of a behavior-object lie, so-to-speak, on that side of it which points towards further means-objects or towards an ultimate goal. They are the ways in which the object, given the manipulanda, or its manipulanda and discriminanda combined, can be useful as a means for getting to further objects and goals. Thus, for example, a behavior-object such as a maze alley which has the manipulanda of run-through-ableness will, as such, also have the utilitanda of leading to objects which are distant in space. Or a behavior-object such as a piece of chocolate will have, by virtue of its manipulanda character as something chewable, the utilitanda character of something which will lead towards a full stomach. Or, still again, the behavior-object, the good-will of another individual, will have, by virtue of its manipulanda character of possessing a substitute pair of hands and feet, the utilitanda character of bringing about the reaching of objects which from the position of the original organism are, as such, unattainable. Or, again, a picture which has both the discriminanda properties of a certain pattern of color and the manipulanda properties of thinness and hang-up-ableness will have the utilitanda-properties of aiding in the establishment of a particular set-up for a certain type of æsthetic satiation.[12]

It is to be noted, however, that in this discussion and in Figure 3 we have again been assuming for simplicity's sake only univocal relations. But such univocal relations do not really obtain. Quite different discriminanda may be coupled on different occasions with one and the same manipulanda. Apples are sometimes red but they are also sometimes yellow.

[12] The terms discriminanda and manipulanda have already previously been used by Tolman (33, 34). The term utilitanda is here, however, now suggested to designate what previously (see especially 34) were called "means-relations."

And one and the same discriminanda will on different occasions be used as signs of different manipulanda. Brown is sometimes coupled with and used as a sign of chocolate but at other times it is coupled with and used as a sign of, say, a negro skin. Similarly, the relations between manipulanda and utilitanda may be equally *equivocal* (*mehrdeutig*). Thus, for example, the run-through-ableness of a maze-alley does probably in somewhat more than 50 per cent of the time have the utilitanda character of getting the organism on towards some further place. But it by no means always has that character, as witness the case of blinds, whose very definition is that they do not thus get an organism on.

A completely adequate diagram of the individual behavior-object and of these its three aspects would have to allow for such internal equivocalities. It would have to be built up, that is, on somewhat the same plan as Figure 4 which we shall come to in the next section.[13]

Finally, before passing on to the next section, we would like here also to point out that just the reverse of the general types of situation allowed for by Figures 2 and 3 also occur. That is, not only are there cases in which the chain between *c* and *d* must be depicted as lengthened, but there are also cases in which this chain is to be conceived as shortened—with fewer, or no, intermediate steps. Thus, in sufficiently primitive, or young, organisms the appropriate diagram would seem to be one in which the arrows in Figure 1 are contracted into a single one running directly from *d* to *c*. That is, in such cases response to cue, manipulation of means and achievement of the goal telescope into but one single process.

For example, Charlotte Bühler and her co-workers Ripin and Hetzer (31) and Rubinow and Frankl (32) have followed the development of the feeding responses in infants. The very

[13] This is perhaps also the place to point out that within the organism there will also be equivocalities as to goals. Professor Charlotte Bühler has pointed this out to us. It leads to such questions as the operation of such fictive goals positive or negative as general "expansion" or general "restriction" of life. [Cf. Charlotte Bühler (6) which with varying degrees of equivocality may perhaps control the more immediate direct goals. See in this connection also the distinction between superordinate and subordinate goals in Tolman, (33) pp. 28 *f.*]

youngest infants responded to the actual touch of the nipple only. But gradually with increasing age the babies began to respond with sucking movements to the laying on of the bib, then later to the approach of any sort of a pointed object. Until finally at about eight months they responded to the presence of a nipple plus a white fluid and to that only. Only at this last stage would the introduction into our diagram of the independent intermediate *hantierbarer Körper b* as in Fig. 1 seem to be needed or appropriate.

We will turn now in the next section to an expansion of Fig. 1 to allow for the sorts of complication which arise upon the introduction of non-univocalities between means-objects and goals and between cues and means-objects.

IV

Figure 1 presented the situation for the organism upon an assumption of univocal couplings of means-Gegenstand to cue-Gegenstand and of means-Gegenstand to goal-Gegenstand. But such an assumption is in reality never realized. The whole uncertainty of knowledge and behavior arises just out of such *equivocality* (*Mehrdeutigkeit*) in the causal surroundings.

Consider for a moment the nature of the causal connections in the physical world independent of organisms. We observe that, whenever any individual event occurs, a more or less extended complex of many independent part causes must have been existentially operative. Further, any specific type of an event will on different occasions and in different places have different causes, or more exactly speaking, different total complexes of part causes. And also, vice versa, any given type of an event will itself operate as a part cause on different occasions and in different places for the production of different final total events. The causal interweavings of unit events among one another are thus, in both directions, equivocal. But some of these connections will be more probable than others.[14]

Exactly this same sort of causal equivocality must be

[14] Concerning the nature of the causal structure of the world in general, see H. Reichenbach (30) and H. Bergmann (1).

applied, now, to the sets of causal chains—those between goals and means and between cues and Gegenstände—in which we are specifically interested. In order, however, not to overcomplicate the discussion we shall consider only a limited number of the actual possibilities.

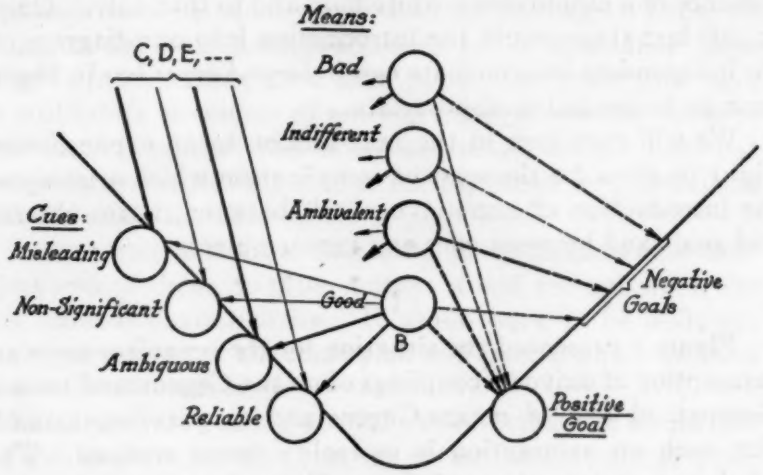

FIG. 4. Paradigm: Four types of goal-means relation; four types of means-cue relation.

Let us examine, first, the right-hand side of Fig. 4. It will be observed that we have depicted one positive goal and several negative goals.[15]

Further, we have shown only four main types of means-object relative to such goals. These four are to be designated as: *good, ambivalent, indifferent, and bad* (*gutes, ambivalentes, indifferentes, schlechtes Mittel*).

The "good" means-object may be conceived as one which, if manipulated, will tend to lead in a relatively high percentage of instances (say up to 95 per cent;[16] heavy arrow) to the positive goal and in only a relatively small number of instances (say 15 per cent; thin arrow) to a negative goal. An "am-

[15] The concept of negative goals is to be conceived here as including not only actually injurious consequences such as real physical injury but also cases which involve, merely, undue expenditures of time or energy in the reaching of positive goals.

[16] The fact that we have chosen examples of percentages which total more than 100 is to allow for the fact that often one and the same type of means-object is capable of leading simultaneously both to the positive and to the negative goals.

bivalent" means-object is to be conceived as a type which will lead with a relatively high probability (*i.e.* with high frequency) to the positive goal but one which may also lead with a relatively high probability to one or more negative goals. An "indifferent" means-object is to be defined as one which will lead with but very little probability, *i.e.* frequency, either to the positive goal or to a negative goal. And, finally, a "bad" means-object is one which will lead with high probability to the negative goal and with but little probability to the positive goal. Finally, we would throw out the suggestion that the "ambivalent" types of means and the "bad" types of means are in some situations (especially, if the negative goals are very intense) to be grouped together under one head and labelled "dangers." For both types will trend to lead with high frequency (heavy arrows) to negative goals.

Turn now to the left-hand side of the diagram. We observe at once a similar analogous lack of univocality. But again in order not to overcomplicate the figure we have depicted only four main types of cue relative to the one "good" means-object, *B*. These four types of cue we have called "reliable" (*verlässlich*), "ambiguous" (*zweideutig*), "non-significant" (*bedeutungsarm*), and "misleading" (*irreführend*). The first type is to be conceived as capable of being caused with great frequency by "other objects" such as *C*, *D*, *E*, etc. The second, or "ambiguous" cue is to be conceived as a type caused with great frequency by both the given object and other objects. The third, or "non-significant" type of cue, is to be conceived as caused with little frequency by either the given object or other specific objects. And, finally, the fourth or "misleading" type is to be conceived as one which may be caused with little frequency by the given object and with great frequency by other objects. Again, we would throw out the suggestion that the ambiguous and the misleading cues may for some individuals and under some conditions constitute a rather special common group to be designated as "hazardous." For both types of cue present a high degree of probability of leading the individual astray, *i.e.* of having been caused by other objects, *C*, *D*, *E*, etc. instead of the to-be-sought for

good means-object *B*. (Hazardous cues would thus be analogous to "dangerous" means-objects).

Considering now both sides of the diagram it appears at once that the psychological success of an organism will depend (i) upon its ability to pick out "good" means-objects for reaching the positive goal and (ii) upon its ability to select the reliable cues for this good means-object. An organism will be successful in so far as it can do both.

But let us indicate the real significance and experimental fruitfulness of these classifications of means and cues by turning to some concrete examples.

V

Let us illustrate, first, the right-hand side of the diagram. We may take a case of learning in rats.

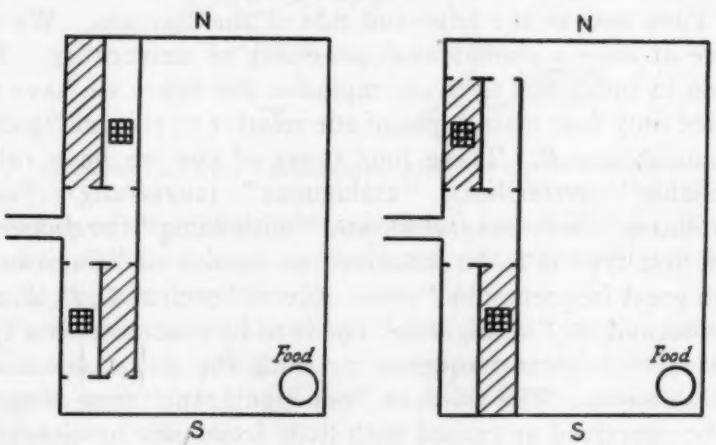

FIG. 5*A* FIG. 5*B*

FIG. 5. Schematic maze for the purpose of illustrating the four basic types of means-object.

Imagine a maze and let us suppose it somewhat unusual in type in that it has choice-points of the two sorts shown in Figs. 5*A* and 5*B*. Suppose, that is, that each choice-point has four alleys, instead of the usual two, issuing from it. Two of these always point south and two north. Further, one alley in each pair is always lighted and the other dark, and one has an electrified grill and the other no such grill. Further, in the

cases of the 5*A* choice-points both the two south-pointing alleys will lead on, whereas both the two north-pointing alleys will be blinds. Also in the 5*A* type of choice-point both the lighted alleys will have electric shocks and the dark alleys will have no shocks. In the cases of the 5*B* choice-points, on the other hand, everything will be just reversed; the north-pointing alleys will lead on and the dark alleys will provide the shocks.

Consider, now, a particular maze in which most of the choice-points are of the 5*A* type and only a few are of the 5*B* type. We see at once that in such a maze, the south-pointing dark alleys are "good"; for they will lead with a high degree of frequency to the positive goal, food, (*i.e.* heavy broken arrow from good means to positive goal as shown on the right-hand side of Fig. 4) and with practically no frequency to either of the negative goals (electric shock) or undue exertion (*i.e.* blinds). (See thin broken arrow.) The south-pointing lighted alleys will be "ambivalent"; for they will lead with a high degree of frequency both to the positive goal, food, heavy broken arrow, and to the negative goal, electric shock (also heavy broken arrow). The north-pointing dark alleys will be "indifferent"; for they will lead with little frequency to the positive goal food (thin broken arrow) and with practically no frequency to the worse of the two negative goals—electric shock, also thin broken arrow. And the north-pointing lighted alleys will be "bad"; for they will lead with little probability to the positive goal, food (thin broken arrow) and with high probability (heavy broken arrow) to both the negative goals, electric shock and blind.

The interesting experimental question is: how will the rats behave in such a maze? Will they pick the "good" alleys and avoid the "ambivalent" the "indifferent" and the "bad"? Obviously the answer will depend upon the nature of their innate propensities, their previous experiences and their stage of learning in this particular maze.

Suppose that all the rats to be used in the experiment have an innate propensity to choose dark alleys rather than light. And suppose, also, that this innate propensity has been re-

ënforced by specific preceding training in a discrimination-box where light alleys always led to electric shock. Suppose, in short, that the rats bring to such a maze a strong "hypothesis," based partly on innate endowment and partly on previous experiences, to the effect that dark alleys, as such, have a greater probability of leading to good consequences than do lighted alleys.

And let us likewise suppose that these to-be-used rats have also all had a preliminary feeding-period in the southeast corner of the room—that is, in the actual spot where the food-box in the maze proper is placed. Also, let us suppose this corner of the room to be in some way distinctly characterized, by virtue, perhaps, of the visual features on the ceiling, or because of odors coming from it, or in some other way. Let us suppose, in short, that the rats also bring a second "hypothesis" to the effect that food lies southward and that south-pointing alleys should, as such, be better than north-pointing alleys.

Rats bringing the above hypotheses and presented to the above sort of a maze should, right from the beginning, and without the need of any new learning, behave relatively "correctly"—*i.e.*, they should at once choose the "good" dark south-pointing alleys most frequently and the "bad" lighted north-pointing alleys least frequently. And presumably they should choose the "ambivalent" lighted south-pointing alleys and the "indifferent" dark north-pointing alleys with some sort of in-between frequencies. It is to be noted, however, there are as yet in the literature no experiments which give exact information as to the two latter sorts of possibility. We do not know, for example, whether the rats will show a greater preference (or lack of preference) for the "indifferent" dark north-pointing alleys, which have only a small probability of being either very good or very bad, or for the "ambivalent" lighted south-pointing alleys which have quite a high probability of being very good but also a high probability of being very bad. The possibility and desirability of further experimentation on such a point as this at once suggests itself. And such future experimentation might

well prove extraordinarily suggestive. It might even prove a way of differentiating emotional dispositions. Thus, for example, the rat who tended to prefer "indifferent" means to "ambivalent" ones might perhaps be defined as "cautious," whereas the one who tended to prefer "ambivalent" means to "indifferent" ones could perhaps be designated as "courageous" or "dare-devilish." And, granting such definitions, then, such a set-up would also allow us to investigate the effects of such factors as degrees of hunger, or varying degrees of having been "blocked" (in Lewin's sense) and the like, upon such emotional states. Indeed, a whole array of possibilities of this general sort for future research suggest themselves.

Or, again, we may turn, now, to the consideration of other types of experiment. These would be experiments in which the total maze would not, as above, agree with the rats initial hypotheses but in which the rats would have to acquire a new hypothesis (*i.e.* to learn). Two sub-types of case present themselves. On the one hand, there would be the type of experiment in which the detailed hypotheses which the rats brought with them were definitely wrong. And, on the other hand, there would be the type of experiment in which the animals brought no detailed hypotheses but merely a very general hypothesis which expressed itself as an initial readiness to explore equally all alleys. (N.B. This latter would be the perfect pure case of trial and error learning.)

To illustrate the former type, let us imagine a situation similar to that previously described save that the actual maze connections would be arranged just oppositely. That is, imagine a maze in which the great majority of choice-points will be like that in Fig. 5*B* and only a few like that in Fig. 5*A*. To such a maze the rats, with their innate propensities and previous experience just as before will bring absolutely wrong hypotheses. What will they do? Obviously they will learn. That is, although they will begin by selecting the objectively "bad" alleys and avoiding the "objectively" good alleys, soon they will begin to correct these initial selections and to acquire

the necessary new hypotheses.[17] But the specifically new and interesting question, which experimentation will be needed to answer, is in what order will the old hypotheses—*i.e.*, the old order of selection of blinds, drop out? What will be the intervening phases of relative preference for the different types of alley through which the animals will pass? Again a whole series of new experiments suggests itself.

Consider now the second sub-type of experiment—that in which the rat brings no specific hypotheses as to north-pointingness nor as to lightedness or darkness, but exhibits merely an initial equal readiness for all four types of alleys, *i.e.*, what we may call the "pure" case of so-called trial and error. What will be the order of learning in such a case? Will the rats drop out the "bad" alleys first, and then the "ambivalent" and then the "indifferent"? Or will they follow some other order? Again important further experiments are needed.[18]

The above must suffice to illustrate the significance, experimentally, of a classification of means-objects based on the probability-relations between such means-objects and goals. We will turn, now, in the next section to illustrations of the experimental significance of the analagous classification of cues, that is, to illustrations of the left-hand side of Fig. 4.

VI

We may imagine a case in which the "good" means-object in the particular instance must possess the property (Gegenstand) of lying at a certain specific spatial distance from the organism. That is, the organism, if it is to be successful, must be able to select correctly the "reliable" visual cues for *third-dimensional depths*. It must be able to distinguish between such "reliable" cues and those which instead are merely "ambiguous," "non-significant," or even "misleading."

[17] The definition of learning as essentially the correction of old hypotheses and the formation of new ones has already previously been suggested by Tolman and Krechevsky (35).

[18] The experiments in the literature which seem already to have made a beginning attack upon such problems as those suggested in this section are those of Hamilton (17a), Kuo (25) and Patrick (27, 28).

As perhaps the best example of *reliable* cues for the visual perception of the third dimension we may take (for organisms with binocular vision) *bi-retinal disparity*. Differences of third-dimensional depth in the environment are projected differently into the two eyes. And the extent and nature of these differences is utilized by the organism as a cue for the perception of distance. But, although such bi-retinal differences do usually stand in almost univocal correspondence with actual differences of distance (relative to the point of fixation), this is by no means always the case. For by means of a stereoscope one can also provoke, as a result of pictures which are really flat, just these same bi-retinal differences. In this latter case the flat pictures produce bi-retinal effects which "normally" are produced only by real differences of third-dimensional depth. But such instances are obviously artificial and exceptional and have but a low degree of "general" probability. They are none the less possible and this possibility is cared for in Fig. 4 by the faint causal line debouching into reliable cues from "other possible objects" *C*, *D*, *E*. . . .

It is interesting to note, further, that the fact that a stereoscope is able to arouse impressions of the third dimension means that the perceptual system as such continues to adhere quite blindly to the hypothesis that bi-retinal disparity is necessarily a cue for third-dimensional depth. And the perceptual system does thus adhere to this hypothesis even when the presence of the stereoscope is an added item among the perceptual data. The perceptual apparatus is, in other words, by itself relatively short-sighted and superficial. It is incapable, at least without specific training, of separating out the case where bi-retinal disparity occurs by itself unaccompanied by a stereoscope from that in which there is the added perceptual data coming from the stereoscope. The perceptual apparatus is incapable of reacting to the former case as indicating with a high probability real third dimension and to the latter as indicating with high probability mere flat pictures. For such a prompt differentiation the superior and more fundamentally accurate processes of discursive thought appear to be required.

Again, let us also note that just the reverse sort of situation can also occur. Bi-retinal disparity cannot only be artificially produced (as by a stereoscope) but it can also be artificially destroyed. Consider, for example, the case in which a scene is observed not directly but in the finder of a camera. In this sort of set-up the effect upon the two retinas is that coming from a flat plane. There is no bi-retinal disparity although there are real differences of third-dimensional depth. In other words, bi-retinal disparity can not only have other causes than real depth in the environment, but real depth can under special, although "normally" improbable, conditions fail to produce bi-retinal disparity. Thus, even for this example of a very *reliable* type of cue, there still obtains some degree of equivocality in both directions,—in the directions both from Gegenstand as cause to cue as effect and from cue as effect to Gegenstand as cause.

Let us consider next an example of *ambiguous* cues for third-dimensional depth. Ambiguous cues we have defined as ones which though also frequently induced by the given type of Gegenstand are of less certain value in that they can likewise frequently result from other Gegenstände than the one in question. An especially good example of such cues relative to third-dimensional distances is *perspective*. Let us explicate. Many objects—especially those common in civilized environments—tend to be right-angled, or to be bounded by parallel lines, or to occur in rows made up of individual items all of equal size. Consider, for example, such objects as: streets, sidewalks, house façades, single windows and rows of windows, corridors, rooms, pieces of furniture and the like. Such single objects or series of objects are, however, very often presented to the organism at an angle, that is, not as face on but as stretching off into a third dimension. Thus it happens that distorted angles and distorted size relations (converging lines and trapezoidal forms, etc.) result with great frequency from differences of third-dimensional depth and the perceptual apparatus comes to use these distorted forms as distance criteria. Much oftener, however, than was true for bi-retinal disparity these distance criteria of "distorted forms" can also result

from other causes than actual third-dimensional depth. Such forms can also be produced with great ease artificially—as for example, by a mere pair of carelessly drawn converging lines. And, likewise, there actually exist in the world many objects whose surfaces are really trapezoidal, diamond-shaped and the like. So that these latter objects even when face on also produce "distorted" images. Indeed many of the familiar optical illusions are cases in which just such "distortions" in actually flat surfaces are responded to as meaning stretching-off into the third dimension.

To return now to Fig. 4 we find this equivocality whereby distorted images can be produced very frequently either by really right-angled objects, rows of equal objects, objects with parallel boundaries stretching off into the distance, on the one hand, or by really distorted objects and artificial objects, on the other, allowed for by the forking of the causal lines which debouch into "ambiguous" cues. And the two branches of this fork are both drawn with heavy lines. That is, in terms of our example, the cue of distorted retinal images may result with about equal and relatively great probabilities either from true stretchings-off into the third dimension, on the one hand, or from actually distorted objects or from mere drawings, on the other.

FIG. 6. Failures of perception due to the persistent functions of hypotheses appropriate only in cases of real perspective.

In other words, a pure reliance upon perspective as a depth criterion necessarily leads to many mistakes (illusions). All the cases where appearances of depth are produced solely by drawings fall into this group. Consider, for example, Fig. 6. The row of three angles appears quite strikingly as a chain of

three mountains really equal in size but extending back into the third dimension. But it is to be noted that when these curves do thus appear, it means that the perceptual apparatus has over-generalized. For in nature there is no very great tendency for rows of mountains equal in size to stretch away from one as so often happens for windows, trees and the like. In fact, the most frequent tendency in nature would seem to be for the nearer mountains (seen as they usually are from the plain) to be actually the smaller. Or, again, consider the other part of Fig. 6. If the two adjacent parallelograms are seen as an open book the perceptual apparatus has again over-generalized. For it has assumed that these angles (and perhaps all angles of this kind) are in nature really right angles. In a word, too great a reliance upon the ambiguous cues of perspective always means laziness and over-generalization on the part of the perceptual apparatus.

Let us consider, now, an example of *non-significant* cues for third-dimensional distances. A relatively good instance is that of *number of intermediate objects*. It appears that the more such intermediate objects there are between an observer and the main object, the further off the latter tends to appear. And in actual nature there is of course some probability that a longer distance will in truth be filled with more intermediate entities than a shorter distance. But there is also some probability that more intermediate objects may mean (result from) something other than greater distance. Intermediate objects will often lie so that the eye cannot detect them. And in any case, it is obvious that in nature there is no such constant relationship between number of intermediate objects and distance as there tends to be between bi-retinal disparity and distance. The cue of number of intermediate objects is then a good example of a relatively *non-significant* cue. (See again Fig. 4.)

Turn now to *misleading* cues. A relatively good example was found in the work of Holaday on the Konstanz-phenomenon in the perception of size (19, p. 454). In this investigation it so happened that in the main experiment, while working with certain definite distances, the left-hand one of

the two objects to be compared (as to size) was at some definitely nearer distance than the other. As a result, it came out that when, in a subsidiary experiment under conditions of poor visibility, the left-hand object was now really the farther away, the subject, because of his preceding training, with the same general range of distances, continued to see it, the left-hand object, as nearer. In other words, the effect of the preceding training in the main experiment perseverated and made the leftness of the given object (under conditions of poor visibility) into a cue for nearerness. Analogous results appeared also in the work of Eissler (**10**, p. 259) on the effect of turning forms out of the frontal parallel plane. Such perseverations (with resulting misleading cues) likewise seem to be frequent and well-known in experiments on weight perception. [See also Izzet, (**20**, p. 316), Brunswik, (**2**, p. 120).]

To sum up for this section, we would emphasize then that, in addition to its task of choosing correct means-objects, the organism has also that of developing an adequate reception system which will tend to select reliable cues, rather than ambiguous, non-significant or misleading ones. And its task is to do this even when all the different kinds of cues are present and competing with one another. The investigation of the degree and manner in which the perceptual system can or cannot do this as well as of its capacity for learning obviously sets the stage for many important further experimental investigations.[19]

VII

In the two preceding sections we have presented examples to illustrate experimentally the different classes of means-objects and cues. And we have seen that these classes are defined by the respective strengths of the causal probability lines between such types of means-objects and the plus and minus goals and between the former and types of cues. Any type of means-object has certain specific probabilities (given the causal structure of the particular environment) of serving as a frequent means for reaching the desired positive goal and

[19] Some beginnings in this direction were in fact contained in the investigations of Holaday (**19**), Eissler (**10**) and Izzet (**20**). For still more recent investigations in the same direction see likewise Brunswik (**2**).

it also has certain specific probabilities of leading rather to one or more of the negative goals. Similarly, any given type of cue has (given the causal structure of the environment) specific, respective, probabilities of having been caused by such and such a Gegenstand or of having been caused by such and such other Gegenstände.

The organism's task is thus, as we have seen, always that of picking out the means-objects and the cues which have the high probability-lines (in the given case) of leading to the required goals and to the appropriate means-Gegenstände. But the next point and the one which we especially wish to bring to the fore in this section, lies in the further fact that the values of these probability-lines are not fixed once and forever for all environments. A means-object, such as a dark alley, which is "good"—*i.e.*, has a strong probability of not leading to the negative goal of injury—in an environment of "free nature" may be "bad"—*i.e.*, have a strong probability of leading to a negative goal such, say, as that of an electric shock—in the special environment of a particular animal laboratory. Similarly, a cue such as bi-retinal disparity, which has a strong probability of having been caused by a true third-dimensional depth in the ordinary environment of hill and dale may have a very small probability of having been so caused in the more special environment of a psychological laboratory which frequently includes, as it does, stereoscopes and other "artificial" devices in front of the eyes.

It appears, however, that an organism usually tends to bring with it to any given new environment a set of already prepared hypotheses. These hypotheses result from its innate make-up and from its previous experiences of "normal" average environments? That is, it will bring with it expectations, based on heredity and early experience that certain types of means-object tend most frequently to serve as causes (routes) to positive goals and that other types tend most frequently as routes to negative goals. And, similarly, it brings hereditarily and from early experience a propensity to expect types of sensory data as having been most frequently caused by certain types of Gegenstände and as having been infre-

quently caused by such and such other types of Gegenstände. But the particular actual environmental set-up may not correspond to the "normal" average environment. God, or the experimenter, may have introduced rather unique and special causal corrections. In such special cases the organism must adjust itself to the new differentiating features and revamp its hypotheses accordingly.

For example, we have supposed that rats, by virtue of innate endowment or of their previous general experience of "normal" spatial environments, tend to bring with them to any maze the hypothesis that south-pointing alleys have, as means-objects, a very great probability of leading towards the south side of the room. But in a given particular maze it may have been established by the experimenter that, contrary to such "normal" probability, the south-pointing alleys shall, in this special case, have a greater probability of leading to the north side of the room and, *vice versa*, that the north-pointing alleys shall in this instance have a greater probability of leading to the south side of the room. It appears therefore that, if the rats are finally to be successful in this particular maze, they must be able to discover further identifying features which differentiate this maze from the "normal" one. And they must attach their new hypotheses to these further features. If they can do this, then when such further features are present, they will react to the south-pointing alleys as having the higher probability of leading north and to the north-pointing as having the higher probability of leading south. And only when such special further features are absent will they revert to the more general hypotheses—suitable for "normal" mazes and "normal" environments in general,—that south-pointing ways have the higher probabilities of reaching the south and that north-pointing ways have the higher probabilities of getting to the north.

Or, similarly, we may suppose that a binocular organism tends (on the basis of innate endowment and early childhood experience) to bring to the perception of the third dimension an hypothesis to the effect that "normally" bi-retinal disparity, as a cue, had a high probability of having been caused

by (and, therefore, of meaning) third dimensional depth. But, in the very special laboratory environments which include stereoscopes, bi-retinal disparity often has a low probability of having been caused by real third-dimensional depth and may become by itself a misleading cue therefore. The binocular perceptual apparatus must, in such a case, correct its initial hypothesis, which was only appropriate for "normal" environments, by including, if it can, within its cue-system the further features as to the presence or absence of a stereoscope. But this, as we saw above, the perceptual system by itself seems unable to do. The organism to be successful must in this situation resort to that more elaborate apparatus which we call discursive thought. That is, in this example the further specifications of the hypotheses needed by the organism for successful immediate adaptations require the cooperation of something more than the purely perceptual apparatus.

Or consider the reverse sort of case. A normally relatively bad means-object for getting to the south side of the room is, as we have said, a north-pointing alley. But under the special "arbitrary" conditions set up by a particular experimenter this round-about route may become a very good means-object for getting there. It appears, indeed, from experiments by Gilhousen [20] that rats which are overtrained on such round-about routes in a special set-up may become so "fixated" on the north-pointing round-about route that they will persist for a long time continuing to try to take it even after it is no longer the correct route. In other words, they can become so overtrained for the special case that when later the special conditions of that case no longer obtain they are unable to drop this special hypothesis.

Similarly, a normally very misleading cue for any specific third-dimensional nearness such, say, as the cue of "being the left-hand one" of two objects, can, as we have seen, under the very special conditions of a particular experiment become a

[20] Gilhousen (13, 14). For other experiments on overtraining and fixation see also Hamilton and Ellis (15, 16), Krechevsky and Honzik (23), Hamilton and Krechevsky (17) Elliott (11) and Everall (12). Indeed, it would seem that what Köhler (24) has designated as "bad" errors (as distinct from "good" errors) are also of the nature of what we are here calling "fixations" resulting from overtraining.

relatively good "reliable" cue. Further, it appeared, however, that the perceptual system can become so overtrained for this special case that when later the requisite special conditions are no longer present the individual may (if the visibility conditions are poor) continue to see the left-hand object as at a certain distance even though it is no longer so. The perceptual apparatus in such a case has also become, by overtraining, so-to-speak "fixated" on lefthandedness as the appropriate and sufficient cue for a certain distance. But if the individual is, under other more "normal" conditions to behave correctly, his perceptual apparatus must be able to abandon this over-fixated hypothesis. The persistence of the latter is an evidence that the organism has become, so-to-speak, lazy and has dropped out some of the essential features of its original "normal" cue system.

To sum up, we may say in general that in the selection both of the means-objects which have high probabilities and of the cues which have high probabilities the organism responds in the form of hypotheses. These hypotheses it brings with it from innate endowment and from previous experience. These hypotheses tend to be correct for "normal" average environments. When, however, the probabilities in the particular environment are not those of a "normal" or average environment, then the organism, if it is not to go under, must acquire new hypotheses. Further, it appears that this new environment may differ from the "normal" either by being more general, or by being more specific, or by being equally, but differently, specific. And still further it appears that in any of these three types of cases the new hypotheses, which must be achieved, require the organism to take into its cue-system and into its selection of means-objects further identifying features. Learning, whether in the perceptual system or in the means-end system, is just such an acquiring of new hypotheses. But, and this is biologically the most important point, such new hypotheses should be attached to the specific identifying features of the particular situations to which they are appropriate. The organism should, that is, become docile to a very developed and subtle

system of sensory cues,—in a way which allows it, for example, to respond differently to one and the same part-cue of bi-retinal disparity according to whether or not the further part-cues presented by a stereoscope are or are not so present.[21] And, similarly, it must also be docile to a very wide and subtle set of means-object differentiations. It must be able, for example, to distinguish the particular north-pointing maze-alley in some particular maze which as such leads south from other ordinary north-pointing alleys which are "normal" and lead north.

Thus the wholly successful organism would be one which brings, innately, normal averagely "good" means-end hypotheses and normal averagely "reliable" perceptual hypotheses; but which can immediately modify these innate hypotheses to suit the special conditions of a special environment; which can note and include in its cue-system and in its means-end-system the presence of the further identifying features of these special environments. But further, such an organism must also, if it is to be completely successful, be equally able at once to drop out such new hypotheses when the special features as to cue or means are no longer present.

In the case of ordinary trial and error learning (whether perceptual or means-end) the new features are noted and the new hypotheses acquired only under the hard task-master of actual bitter behavior. In the case of "insight" learning the new features are noted and the requisite new hypotheses are

[21] Or to take, perhaps, a better example for this case of becoming docile to a very developed and subtle system of sensory cues, it appears that this is just what has happened in the case of the so-called Konstanz-phenomenon in the perception of size, color and the like—thus, for example, to take the case of size-perception, it appears that the organism has developed an extraordinary ability and propensity to perceive, as intentionally attained Gegenstand, the "real" size of an object independently of enormous differences in the size of the visual angle which this object presents to the eye when at different distances away. But this means simply that the organism has come to include in its cue system visual angle plus one or more reliable distance criteria. Every type of perceptual "*Ding-konstanz*" depends in fact upon just such a mutual working together of a variety of cues (*e.g.*, direct retinal effects of size, color, etc.; distance criteria; direction criteria; illumination criteria, etc.) *Cf.* in this connection the discussion of Brunswik and Kardos (4) of the "Zweifaktorenansatz" of K. Bühler and the considerations concerning the equivocality of single stimuli by Heider (18) and also by Brunswik and Kardos (4).

evoked as a result of innate endowment and general experience before they have ever actually specifically—behaviorally—been put to the test. In the case of unmodifiable instinct the new features are never noted and the new hypotheses never acquired; the organism continues to behave in the old fashion and goes under. In the case of motivational and emotional inadequacies the organism is either overhasty or overlazy in making observations of the new cue-features and the new means-features and in developing the requisite new more adequate hypotheses.

Indeed, we would like to throw out here, as a final word, the suggestion that all the problems of psychology—not only those of visual perception and of learning—but all the more general problems of instinct, insight, learning, intelligence, motivation, personality and emotion all center around this one general feature of the given organism's abilities and tendencies for adjusting to these actual causal textures,—these actual probabilities as to causal couplings.

VIII

In conclusion, we would summarize as follows:

1. The environment of an organism has the character of a complex *causal texture* (*Kausalgefüge*) in which certain objects may function as the *local representatives* (*die Stellvertreter*) of other objects; these latter to be known as the *entities represented* (*die Vertretenen*).

2. This function of local representation has, however, two subvarieties.

(*a*) On the one hand, objects or situations may function as local representatives of others in that they provide (with the cooperation of the organism) *means-objects* (*Mittelgegenstände*) to the others; these latter to be known as the *goals* (*Zielgegenstände*).

(*b*) On the other hand, objects or events may also function as local representatives for others in that, being themselves caused by such other objects or events, they serve as cues (*Anzeichen*) for the latter. These latter in their turn would then be known as the *Gegenstände* relative to such cues.

3. The simplest paradigm involving these two kinds of local representation will be one in which an organism is presented with a single *behavior-object* (*hantierbarer Körper*). This behavior-object is to be conceived as lying "in between" the *need-goal side* (*Bedarf-Erfolg-Seite*) and the reception-reaction-side of the organism. And, as so lying, it may function causally in two ways:

(*a*) This object can (with the cooperation of the organism) function as the means-object for the reaching of some goal.

(*b*) This object can also send out causal trains which may be picked up as cues by the reception-reaction-side of the organism. These cues will then function to represent the Gegenstände which make up the object.

4. These resulting cues, considered as a reactional event, may be said anticipatively to "*lasso*" (*lasso-principle, i.e., sign-gestalt*) the present causal complex on the basis of past causal complexes. In other words, such cue-Gegenstände will be responded to as presenting then and there an actual instance of the given type of means-Gegenstand and as also presenting (transitively) through this means-Gegenstand the possibility of such and such a final goal-Gegenstand.

5. But such a paradigm with only one behavior-object between goal and cues is for some types of situation too simple and for others too complex.

(*a*) In many actual situations there may be more than one successive means-object and more than one successive cue-object. But such cases, although the picture must be complicated to allow for them, do not introduce anything new in principle.

(*b*) It also appears that such a single intervening behavior-object (Gegenstand-complex) may have three, somewhat independently variable and distinguishable, aspects. These are to be designated as its *discriminanda*, its *manipulanda* and its *utilitanda*. These further complicate the picture but they do not demand anything fundamentally new in principle.

(*c*) Finally, there are other types of situation, obtaining for very young or for very primitive organisms, in which there are no distinct intervening Gegenstände, as such, between cues

and goals. The whole picture must in such cases be conceived as telescoped.

6. It appears now, further, that the causal couplings between goal and means or between the latter and cue (or between different aspects within any one of these) are seldom, if ever, *univocal* (*eindeutig*). For it appears that any given type of goal will be capable of being causally reached by more than one type of means-object. And, vice-versa, any given type of means-object will be capable of leading to more than one type of goal. Similarly, any given type of means-object can cause more than one kind of sensory cue and any one type of cue can be caused by more than one type of means-object.

7. Such *equivocality* (*Mehrdeutigkeit*) brings it about that the organism has to venture hypotheses as to what the given means-object will "most probably" lead to in the way of goals or as to what type of means-Gegenstand the given cues have with "most probability" been caused by. (Such hypotheses are always capable of purely objective definition.)

8. Further analyses of the actual types of probability-relation which may obtain suggest preliminary, and it would seem experimentally fruitful, classifications of means-objects into the four types: *good, ambivalent, indifferent and bad* (*gutes, ambivalentes, indifferentes and schlechtes Mittel*) and of cues into the four types: *reliable, ambiguous, non-significant and misleading* (*verlässliches, zweideutiges, bedeutungsarmes* and *irreführendes Anzeichen*).

9. It appears that the organism's task in any given case is to correct whatever hypotheses it brings with it to fit the real probabilities of the actually presented set-up.

10. The organism brings hypotheses based on innate endowment and previous experience which tend to be suitable to the probability-relations of "normal" environments. But in any actual given environment these "normal" probability-relations may not hold.

11. If, therefore, it is to be successful, the organism must eventually develop both cue-systems and means-object systems which are, at one and the same time, both wide and inclusive and yet full of very fine discriminations.

12. Finally, it appears that the study of the organism's abilities and propensities in the development and operation of such cue-systems and mean-end systems and resultant hypotheses involves not only the problems of perception and of means-end learning, but also those of instinct, memory, insight, intellect, emotion—in short, perhaps, all the problems of psychology.

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THE PRESENT STATUS OF ABNORMAL PSYCHOLOGY

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Abnormal psychology is and has been for some time, a somewhat amorphous mass of ill-defined and often poorly related or totally unrelated subject matter. Its boundaries seem largely to depend upon the whims of the teacher or author dealing with the subject. Few psychologists have attempted to survey the field in an objective way, or to relate known experimental knowledge to the subject matter of their texts. Nor has there been an objective examination of the part that controlled experimentation plays. Although it is an established, historical fact that psychology has made little progress except through the use of the experimental method, abnormal psychology today is top-heavy with theory. The basis of experimental evidence is relatively small in contrast to the immense superstructure of theoretical hypothecation that burdens the subject.

The present writers desire to survey the field of abnormal psychology and to evaluate the role of the experimental method in the field. For this purpose we have examined and classified the content of fifteen textbooks of abnormal psychology which have appeared during recent years. Since the textbooks represent the work of standard authorities in the field, their inclusions and omissions should give a representative cross section of the subject. In addition, their tremendous influence in the teaching of abnormal psychology tends to stabilize the field along the lines of their presentation. For this reason, because as textbooks they both reflect and influence, the writers feel that an objective survey of their

material can give a fair and reliable picture of the present content of the subject. For comparative purposes we have analyzed, in similar fashion, a small group of representative psychiatric texts.

We have selected for our survey the texts of Abnormal Psychology by the following authors: Bridges, Conklin, Coriat, Fisher, Hart, Hollingworth, McDougall, Meyer, Morgan, Moss and Hunt, Murphy, Myerson, Pillsbury, Pressey, and Taylor.

As a comparison group psychiatric textbooks by the following authors [1] were chosen: Bisch, Bleuler, Henderson and Gillespie,[2] Henry, Rosanoff, Strecker and Ebaugh, and White.

The following 24 classificatory categories were chosen as being most suitable for our purposes in dealing with the abnormal psychology texts: Psychoses, Disorders Primarily Physiological (epilepsy, paresis, etc.), Psychoneuroses, Perversions (sexual aberrations), Disorders of Sensation, Emotion, Intelligence (both amentia and genius), Memory, Personality (including motor automatisms), Speech, Hypnosis and Suggestion, Drugs, Sleep and Dreams, Varieties of Consciousness (co-conscious, unconscious, etc.), Psychological Types, Normal Individuals, Psychoanalysis, Psychotherapy, Etiology, Mental Hygiene and Prevention, History, Theory, Experiments (direct treatment of the results of experimentation as such), and Miscellaneous. In dealing with the *psychiatric* texts, the list was changed by omitting the categories of Speech, Hypnosis and Suggestion, Drugs, Sleep and Dreams, Varieties of Consciousness, and Experiments. It was also necessary to add categories for Examination and Case History, Symptomatology, and Classification.

The selection of the above classes was of course largely arbitrary, but the writers felt that these best covered and

[1] It was felt that Hamilton's "Objective Psychopathology," Mosby, St. Louis, 1925, properly belonged in any group of representative psychiatries, but its novel plan and arrangement plus its mass of case studies made it impossible to classify under any comparable system.

[2] There is a later edition of Henderson and Gillespie, strictly comparable to this in relative stress, but with the addition of 49 pages on the "Psychiatry of Childhood."

differentiated the material of the texts. In applying this classification, it was again necessary to be arbitrary. We believe that through the exercise of discretion and judgment it has been possible to present a fair picture of the content and relative stress of the books. In cases of overlapping, where material might possibly have been placed under any one of several categories, the writers have assigned it to that category primarily stressed in the treatment. Thus the various amnesias are treated independently in certain of the abnormal texts but we have classified them under Disorders of Memory. In the psychiatric texts the amnesias are almost wholly stressed as symptoms of larger, more generic disorders and properly come under the heading Symptomatology. Material on multiple personality is sometimes treated as a disruption of personality, an attendant phenomenon of hypnosis, or from the standpoint of the co-conscious, as a variety of consciousness. The ultimate classification has depended upon the aspect receiving the greatest stress. This system of classifying material exclusively under a single category seems to offer the best means of comparison between texts in a tabular presentation. The content has been evaluated in the following tables as to percentage of the total number of pages in each book which this represents. Table I contains the classification of the 15 texts in abnormal psychology, while Table II deals with the texts of psychiatry.

An analysis of Table I enables us to draw some interesting conclusions regarding the material and treatment of abnormal psychology as represented in the current texts.

The first thing that strikes us is the divergent nature of much that is included in the field. Abnormal psychology serves as a waste basket for many of the odds and ends which cannot be fitted in elsewhere. It is a convenient pigeon-hole for filing away much that is found there simply because it does not fit elsewhere. Why is there a frequent treatment of sleep, a normal natural phenomenon, under this heading? What is the justification of viewing all dreams as abnormal? Some authors treat synæsthesia as a part of the field. Genius is frequently included. The size of the figures in the "Mis-

TABLE I

PERCENTAGE OF MATERIAL IN VARIOUS ABNORMAL PSYCHOLOGIES DEVOTED TO VARIOUS TOPICS

	Bridges	Conklin	Coriat	Fisher	Hart	Hollingworth	McDougall	Meyer	Morgan	Moss & Hunt	Murphy	Myerson	Pillsbury	Pressey	Taylor	All texts
Psychoses	5	5		11		4	7		9	5	23	6	11	10	3	7
Disorders primarily physiological	9	2				2	2	2	1	24		6	4	10	1	4
Psychoneuroses	6	6	20	17		19	5	21	6	2	7	6	9	8	13	10
Perversions	1			5			2		2				4			1
Disorders of Sensation	12	5	4				2	3	14	3		6			5	4
Emotion	5		11						3	1		2		2	1	2
Intelligence	3	7		8		10			6	3	8		9	7		4
Memory	2	4	9					1	5	1						2
Personality	7		7	16	40	4	20	5	15	1	11	4	5		16	10
Speech						4			1		5		4		1	1
Hypnosis and suggestion	1	6	5	6			8	9	6		2		11		7	5
Drugs	1	5				5	2	3		5		2				2
Sleep and dreams	2	11	14	8		1	4	2	4				4		4	4
Varieties of consciousness	3		7	2			1				4				2	1
Psychological types				3		2	3		3		1		6		1	1
Normal individuals		6		6				18	8					6	12	4
Psychoanalysis	2	8	18	6	39	8	24		1	2	7	10	10		10	9
Psychotherapy		8					4	5	1	5		2	1	5	4	3
Etiology	4	10		4	6	19	3	1	3	11	4	20	6	4	2	6
Mental hygiene and prevention			3				1		5	5	11	7	5	7	1	3
History		5		1	6	8			3	4			4		1	2
Theory	3				6	3	8	10	2	13		7	4	4	6	4
Experiments						5	1			4					1	1
Miscellaneous	34	12	2	7	3	6	3	20	2	11	17	22	3	37	9	10
Total No. of Pages	236	457	428	512	176	590	572	278	627	548	331	135	375	356	789	6410

cellaneous" column is due not so much to individual treatment which resists classification as it is to the inclusion of extraneous material of various sorts. We find in these texts such widely divergent topics as juvenile delinquency, crime, stage fright, spiritualism, anatomical abnormalities and atypical physiological and chemical conditions. The lack of orientation and definite purpose, encouraging as it does the inclusion of the particular enthusiasms of some of our authors, results in jumbled disorder and lack of clear definition of what the subject matter of abnormal psychology is or should be. In view of the tremendous influence of textbooks in teaching, such an absence of clarity and the consequent failure to define the subject matter is lamentable. It cannot help but have an unhealthy effect upon progress in the field.

This absence of orientation no doubt has led to the second characteristic in the prevailing treatment of abnormal psychology. The material is often presented rather as a chamber of horrors than as a coherent attempt to understand, explain, and control the problems of abnormal behavior. Hallucinations, delusions, automatisms, delirious flights, somnambulisms, etc., are often treated independently as individual entities, and in purely descriptive fashion, without the attempt to fit them into their functional relations in the whole picture.

A comparison of the relative amounts of space allotted to some of the particular topics brings out further interesting facts. Despite the hue and cry over psychoanalysis, it does not seem unduly stressed in most of our books. In fact, in view of its influence and the number of its adherents, serious criticism may be leveled against the scanty treatment accorded it by some authors.

Of the 13 abnormal texts which include a treatment of psychoanalysis, an average of 11.5 per cent of the pages of these texts are devoted to the subject; of all 15 texts, 9 per cent. Park has shown that a treatment of psychoanalysis constitutes 4.07 per cent of the text material of present day elementary or general psychology. There is no question but that psychoanalysis, which is an outgrowth of psychopathology or abnormal psychology, has had more influence in the

formulation of psychological thought in this field than any other single factor. That it is included to the extent of 4 per cent in general psychology shows that it has colored the entire field, and that it enters into only 9 per cent of the text of abnormal psychology seems to us to constitute a distinct shortcoming, no matter what one may think of the actual significance of psychoanalytic theories.

The attention devoted to etiology (5 per cent) seems remarkably small. True, there is not a great body of definite knowledge concerning causation, in spite of its importance, but since we can find plenty of space devoted to more hypothetical matters, perhaps even less securely based upon definite facts, there does seem to be justification for concluding that etiology is not given the treatment it deserves. There also seems to be some basis for complaint over the scant space allotted to psychotherapy (2.4 per cent), and to hygiene and prevention (2.7 per cent). Certainly, these are of great practical importance. Whether or not their pragmatic value to the individual and society justifies a fuller discussion depends upon one's conception of the subject and the pedagogical purpose of the texts. It may in all fairness be pointed out that they have theoretical and abstract implications as well as pragmatic value. After all, the discussion raised on this point may be blamed on the somewhat haphazard conception of the subject matter of abnormal psychology. The more one surveys the field the more one is forced to conclude that future progress depends to a great extent upon a careful mapping of the ground and a rational plan of experimental development.

One final and definite criticism that can be raised as a result of scanning our table is the neglect of experimental treatment in the texts. This neglect of the experimental approach (only .8 per cent) seems the most glaring shortcoming in our texts. In view of the tremendous importance of experimentation in any science its absence in the treatment of abnormal psychology is alarming and lamentable. While it is true that some of the matter presented consists of experimental results, they are not treated as such and one seldom finds any discussion of conditions and controls. In short, the importance

given to the experimental method and experimental programs in our texts of normal psychology is sadly lacking here. We will return to this point later.

Table II offers us a comparable view of seven representative psychiatries. They are of interest to us primarily as they

TABLE II

PERCENTAGE OF MATERIAL IN VARIOUS PSYCHIATRIES DEVOTED TO VARIOUS TOPICS

	Bleuler	Bleuler	Henderson & Gillespie	Henry	Rosanoff	Strecker & Ebaugh	White	All texts
Psychoses	1	15	26	10	8	22	18	15
Disorders primarily physiological	9	15	16	12	12	27	18	16
Psychoneuroses	5	5	8	5	4	11	4	6
Perversions		1			2		1	1
Disorders of Sensation		7						1
Emotion		6		2				1
Intelligence	10	3	5	2	5	2	2	4
Memory		2						1
Personality		1		5		2		1
Psychological types					3			1
Normal individuals	6	8		14				3
Psychoanalysis			4	2	3	1	2	2
Psychotherapy		5	6	4	6	3	4	4
Etiology	3	4	11	3	5	2	5	5
Mental hygiene and prevention	3			7	2			1
History	2		2	3				1
Theory		2			3	3	7	2
Examination and case history	7	1	3		2	5	12	4
Symptomatology		20	8	3	10	2	18	10
Classification	3	3	2	2	2	2	1	2
Miscellaneous	51	2	9	26	33	18	8	19
Total No. of Pages	346	635	520	199	697	553	345	3295

afford a comparison for the abnormal texts. We notice immediately some of the same diversity of material, since we find treatments of psychoses and war, childhood, nursing, social service, sociology and psychiatry, association and intelligence tests, moral deviations, etc. Here, however, the diversity is nowhere near as great and the material is much more closely related. The psychiatries are in marked contrast to the abnormal texts inasmuch as they are built around

a more definite plan. The subject matter is knit fairly closely around the recognition, understanding, and treatment of mental disease. The definiteness of content and purpose is quite apparent. There is little inclusion of borderline behavior, no treatment of hypnosis, dreams, and difficulties of speech as problems in their own right, and of course there is a much greater stress on physiological disorders. This contrast suggests the possibility of two types of subject matter; one the problems of the overt forms of abnormal behavior, the psychoses and psychoneuroses, both organic and functional; the other the problems of the borderline types of behavior, peculiarities of personality, slips of speech and of memory, the intricacies of hypnosis and suggestion, etc. It may be that the relative directness and unity of the psychiatries derives from their preoccupation with the first. It may be that the lack of unity and coherence of the abnormal psychology texts derives from their attempt to deal with both. Whatever the reason, there is this contrast in the clarity and unity of their structures.

The psychiatries, however, are no better than the abnormal psychologies in their treatment of the experimental approach. In an endeavor to shed further light on this lack of experimental material, the writers have carried their investigation a bit further afield. Table III represents a tabulation of those

TABLE III

A. P. A. Members in Abnormal and Experimental Psychology

Instruction		Research	
Abnormal 130	Experimental 160	Abnormal 211	Experimental 480
Both 16		Both 44	

members of the American Psychological Association who are listed in the Yearbook for 1933 as teaching and as doing research in the fields of abnormal and of experimental psy-

chology. It will be seen that while the number teaching experimental psychology is only about 20 per cent greater than that teaching abnormal, the number doing research in experimental is over 100 per cent greater than the number engaged in doing research in abnormal. Nevertheless, the absolute number of investigators doing research in abnormal psychology is sufficiently large to warrant more attention to their productions in the text-books.

The writers have also investigated the experimental literature in the journals of recent years. The Psychological Abstracts for 1929, 1930, 1931, 1932, and 1933 were perused and a careful count was kept of all articles under the 3 headings, Feeling and Emotion, Special Mental Conditions, and Nervous and Mental Disorders, that reported the definite use of the experimental method in the problems of abnormal psychology. Only those using more than a single subject were counted, and purely statistical surveys were omitted. Once again, as in any such classification, it was necessary to be arbitrary, but the writers feel that the result is an adequate picture of the status of the experimental approach in the field of abnormal behavior. In Table IV there are reported for each of the four

TABLE IV

EXPERIMENTS REPORTED IN ABNORMAL

	Total Abnormal Experiments	By A. P. A. Members	Total Articles in Abstracts	Total Articles Under 3 Special Headings
1929	53	11	5,016	1,095
1930	72	19	5,139	1,258
1931	82	22	5,066	1,080
1932	114	23	5,088	1,241
1933	124	30	6,129	1,491

years the number of experimental articles in abnormal psychology, the number of these by members listed in the 1933 Yearbook of the American Psychological Association, the total number of articles in each year's Abstracts, and the number under the selected three headings given above. In Table V the total number of experimental abnormal articles

TABLE V

EXPERIMENTS IN ABNORMAL

	In Terms of Percentage of the Total No. of Articles in the Abstract		In Terms of No. of Articles Under 3 Special Headings	
	Total Abnormal Experiments	By A. P. A. Members	Total Abnormal Experiments	By A. P. A. Members
1929	.011	.002	.048	.010
1930	.014	.004	.057	.015
1931	.016	.004	.076	.020
1932	.022	.005	.092	.019
1933	.020	.005	.083	.021

and those by A.P.A. members for each year are expressed in terms of percentages both of the total number of articles in the Abstracts for that year and of the number under the chosen special headings. The lamentably small number of actual experimental publications in the field shows up definitely both in numbers and percentages in these tables. The one encouraging fact is that there is a small but continuous improvement to be noted from year to year (possibly excepting 1933). This small experimental production may possibly be responsible for the lack of experimental treatment in the texts. In turn the failure of the texts to stress the experimental approach may be partly responsible for the slight work turned out. However, whatever the reason, the almost complete neglect of the experimental approach in our standard texts is extremely unfortunate. Small as the relative amount of experimental work in abnormal psychology may be, the absolute production is sufficiently large to merit a great deal more attention in our texts on the subject, particularly in view of the extreme importance of the experimental method in the development of science.

The entire discussion which we have raised really revolves about two major questions and one minor question; namely, just what should be the subject matter of abnormal psychology; just what experimental work is relevant to the subject; and to what extent experimental work in this field is feasible.

We have indicated that there seemed to be two major lines of classification which run through both abnormal psychology and psychiatry; namely, the borderline phenomena of behavior and experience which may be considered as abnormal from either a statistical or a social point of view, and the phenomena of pathological behavior and experience found in the psychoses and psychoneuroses. Although most of the material can be subsumed under these two major lines, there has been a continual attempt to deviate from them, through the addition of material which is not really relevant. It seems to us that the determination of whether or not these two major lines of subject matter are really closely allied is a crucial point in psychology. As a matter of actual fact they may have little or nothing to do with each other. There really is little or no experimental evidence available which indicates clearly that the psychological deviations of behavior, which would be classified under such headings as introversion, feeblemindedness, juvenile delinquency, dream analysis, synæsthesia, slips of the tongue, etc., are really related to the problems of the psychoses and the psychoneuroses. This question has been theoretically discussed many times, but this discussion has not led to pertinent experimentation. It is our present belief that these two lines of phenomena have basically little in common, but we must await further evidence.

The problems of experimental work in psychopathology are more difficult than is usually recognized by the theorist or by the strictly academic psychologist. After all, most of the investigations must make use of individuals who are both mentally and physically sick. They have no interest in the psychological experimentation. Their point of view is the extremely personal one of any sick man. In addition, the psychopathic patients are extremely heterogeneous with respect to such factors as age, education, interest, manual dexterity, etc., which are usually controlled in psychological experimentation. In spite of these difficulties a respectable body of knowledge is at present to be found in the various scientific journals, reporting many interesting and illuminating facts which have been brought out by experimental proce-

dures. It is our contention that even though these facts are all too frequently isolated and have not been reported in a fashion which relates them to the field in general, they should receive more attention than they have. We believe that an adequate survey of the work which has been done in experimental psychopathology during the past 40 years will bring about an entirely new point of view in the realm of abnormal psychology.

To summarize: our analysis and tabulations show that the field of abnormal psychology has at present a body of experimental knowledge which has not been utilized to any extent by the authors of the text-books. Many of the text-books contain collections or systematizations of material which is frequently irrelevant and morbid rather than pertinent and scientific. If this section of the field of psychology is to assume its rightful place in the science, it will be through the formulation of the subject as an experimental science rather than as a theoretical summary of some armchair philosophy.

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MANIC-DEPRESSIVE PSYCHOSES OF BUSINESS [1]

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Emerging as we are from a very extreme business depression, with the worst probably over, we should be able to make a relatively cool analysis of the symptoms which were manifest while business was in the midst of a major phase of a manic-depressive psychosis.

This paper proceeds from the premise that business is subject to various psychoses which need analysis by psychologically trained persons and, in support of this premise, proposes the consideration of business depressions as illustrative of a psychological interpretation of one syndrome.

Looked at from close range there is a temptation to believe that what we have witnessed is an isolated event due to some sort of calamity which produced a disruption of normal business. Taking a sufficient period of time to give us an adequate perspective, it is apparent that business has gradually been developing a functional instability which is growing into more and more clean-cut demonstrations of psychotic symptoms which are strangely similar to the symptoms of manic-depressive psychosis in the individual.

Whatever our diagnosis may be, we must admit that business has been sick and we must also admit that there is profound ignorance as to the cause, the significance of the symptoms manifested, the treatments that should be applied, and the prognosis for the disease. As in all cases where ignorance abounds we have no dearth of theories to explain the various symptoms. We have all sorts of remedies suggested, most of them quack remedies; indeed one person has called this the quack remedy stage of modern business.

It is well known, in the case of a manic-depressive psychosis

[1] Presidential address read before the Midwestern Psychological Association at Purdue University, May 11, 1934.

in an individual, that the patient usually recovers in spite of any and all treatments to which he is subjected. After such recovery every person who had anything to do with the patient can lay claim to credit for the cure and, doubtless, after a complete recovery from the recent depressed phase of the business cycle, we shall hear all sorts of claims as to the efficiency of certain measures which were used. The history of modern business leads one to believe that there would have been a recovery no matter what had been done. The recovery of the patient is never a valid argument in favor of the treatment given.

Consequently, our aim is not to suggest some new quack remedy, but to make an analysis of some of the symptoms. We shall attempt to show that business is suffering from a functional psychosis which has its roots in the emotional conflicts of the business organism. Our method will be the case study method.

It is fairly well agreed that the depression through which business is just passing is not an isolated symptom but a phase of a more or less rhythmical up and down movement which has been given the name business cycle. Furthermore, the characteristic business cycle as we know it today, with its wild speculation and abnormal depressions, has been apparent only since the development of an elaborate money economy.

From the middle of the eighteenth century to the present day, business has shown a cyclical character in which industry rises to a high stage of activity, or boom excitement, until a crash ensues; then there is a period of unemployment, business depression, often culminating in a panic; a recovery, business activity, and finally another boom. With the entrance of the seventeenth century, clear-cut crises as an aftermath of inflation may be found.

Since 1873 we have been becoming more and more business cycle conscious, so much so that newspapers glibly inform readers just what phase of the business cycle we are in at any moment. Governmental agencies have been used to study the causes of business cycles so that steps can be taken to control them. Indeed, before the last business collapse we

were told that the whole phenomenon had been analyzed and that the Federal Reserve Board had developed means for controlling cycles. We were in a "new era" and would see no more industrial crises. Such prophetic utterances were nullified by the outbreak of the worst business depression of the century.

Business Cycle Theories

A great wealth of theories have been advanced to explain business cycles. There have been a number of meteorological theories; solar cycles, sun spots, and magnetic perturbations have been correlated with business cycles with questionable results. Some writers attempt to give logical reasons for such supposed relationships. One argument is that meteorological changes affect the weather and weather affects crops. However, unfortunately for the theory, correlations of business cycles with crops have not been very high. A still more ingenious attempt to discover such a relationship was developed in 1919 by Huntington. He thought he found that a high death rate regularly preceded hard times, while a low death rate preceded prosperity. He concluded that "business cycles appear to depend largely on the mental attitude of the community . . . the mental attitude depends on health . . . and health depends largely upon the weather."[2] He may have been right in his first assumption that business cycles depend upon the attitude of the community but he probably would have done better if he had then attempted to analyze that attitude and its influences rather than to try to shift it back to such tenuous relationships as death rates and weather.

Another group of theorists see business cycles as a natural outcome of the adjustment of business to new developments, or to progress in general. Scientific discoveries, innovations in production and marketing, exploitation of new resources, shifting of trade routes, and the like, all involve a continual readjustment. Such readjustments do not proceed evenly. The combination of qualities needed for business success in all this welter of forces is rare and only occasionally does a genius arise who transforms business. His success brings in its wake

[2] Ellsworth Huntington, World-power and evolution, New Haven, 1919, p. 42.

a group of lesser men who follow his methods and a great wave of prosperity follows until it gradually spends itself, the wave subsides and depression persists until some new genius revives it. In short, these theorists make business cycles an indirect result of the differences in intelligence of business men.

In 1894 a Russian by the name of Tugan-Baranovski explained business cycles as the result of scarcity of capital.[3] During prosperity, investments take such a large proportion of capital that funds become exhausted. Borrowers are unable to secure funds to finish their financing and the boom ends in a crisis due to this stringency of capital. The shrinking of the loan funds is accentuated because workers no longer are able to save. However, those on fixed incomes continue to save and in time their savings make up sufficient reserves to capitalize new enterprises and business starts again on its forward movement.

An English writer, John J. Hobson, gives an explanation which is flatly contradictory to that of Tugan-Baranovski.[4] He says that cycles are caused by over-saving. His explanation goes somewhat as follows: In modern society the great mass of production is accomplished by relatively few individuals, whose incomes increase more rapidly than the average. This wealthy class seeks to invest its new savings in productive enterprises and consequently brings about an over-supply of goods. This excess grows until actual congestion makes selling impossible. Factories close, prices fall, capital is written down and the incomes of the wealthy class are thereby brought into line with what they should be and the foundation thus laid for another wave of productivity, over-saving, and over-capitalization. Over-saving and over-production theories state in a fairly clear manner what happens in business cycles but they leave one with a feeling of inadequacy. One feels that he has a description of symptoms but a description which does not get to the causal elements in the disease.

An American by the name of George H. Hull [5] applies the

[3] See Wesley C. Mitchell, Business cycles, Nat. Bur. Econ. Res., 1930, pp. 23–24.
[4] *Ibid*, pp. 24–25.
[5] George H. Hull, Industrial depressions, New York, 1911, pp. 103–107.

over-production theory to construction work and makes this one specific factor in modern business the cause of the business cycle. He argues that only about two-thirds of construction work is necessary; that is, we need only a limited amount of repair work, replacements, and new construction to keep pace with a growing population. The remaining one-third is optional. When costs of construction are low enough to arouse the "bargain-counter" instinct some far-seeing individuals and a great many more who cannot see very far begin construction projects. This wave of building grows until there is a veritable scramble for material and workers; prices rise, and the aim seems to be to complete various projects before competitors can complete theirs. This stress upon speed shortens the foresight of people and over-building results. When this condition becomes apparent, construction stops precipitately, those employed in building industries are discharged and this starts the downward movement toward depression.

Colonel Malcolm C. Rorty [6] has argued that the essential element in this theory of over-building lies in the over-commitment element. He says that each new commitment extends credit, which credit is used as purchasing power and inflates the credit structure. When these new projects surpass the normal trend of growth, and this over-extension becomes evident, it is too late to recall the credit without starting a precipitate downward movement. With this introduction of the significance of the over-extension of credit, Colonel Rorty is touching a psychological factor which seems to be significant for us and to which we shall return.

Professor Irving Fisher [7] of Yale University contends that the primary cause for business cycles is what he calls the money illusion. We deceive ourselves, he argues, into thinking that we have a stable dollar because we keep the gold content of the dollar the same. The real value of the dollar is its purchasing power. Taking the purchasing power of the dollar

[6] Malcolm C. Rorty, Some problems in current economics, Chicago, 1922, pp. 73–84.

[7] Irving C. Fischer, The money illusion, N. Y. Adelphi, 1928; also Booms and depressions, N. Y. Adelphi, 1932.

in 1913 as a base, he shows that the value of the dollar rose from 60 cents in 1865, to $1.43 in 1895, then decreased to 44 cents in 1920.

Various circumstances operated to cause this marked and almost continuous inflation from 1895 to 1920. Between 1896 and 1914 new gold mines were opened. The cyanide process for increasing production of gold was introduced. Gold poured in from Colorado, Alaska, Canada, and South Africa. The war from 1914 to 1918 was accompanied by paper money inflation in Europe. We demanded payment in gold and there was a great inpouring of gold from Europe. In addition, we increased our credit money and issued billions of dollars worth of Liberty Bonds, thus decreasing still further the value of the dollar until it reached the low value of 44 cents when compared with the 1913 dollar.

Fisher shows the evil effects of this fluctuating value of the dollar in mystifying the business man and in tending to undermine his confidence. This destruction of confidence comes primarily because we all tend to deceive ourselves into thinking we have a stable dollar; we make contracts and plan our lives as though the dollar were stable, when it is extremely unstable. How does this self-deceit operate?

The man who saves during a period of inflation is deceiving himself, for the value of the dollar may decrease so that at the end of a saving period he may find that his principal and interest combined will buy less than the dollar would have purchased had he spent it at once. For example, "A workingman who, in 1896, had put one hundred dollars into a savings bank found, in 1920, that his principal, plus compound interest at 4½ per cent, amounted to about three hundred dollars. On the face of it, he had his principal back and in addition $200 of profit, or accumulated interest. This $200 seemed a genuine reward of thrift. But it was an illusory profit. . . . When our American depositor came to spend his $300 in 1920 he found prices nearly four times as high as they had been in 1896; consequently his entire accumulation of $300 would buy only about three-quarters as much as his original $100 would buy in 1896. He would have come off better if he had spent

his hundred dollars in 1896. If he had turned his $100 into furniture, jewelry or some other representative commodity in 1896 and simply held that commodity until 1920, he would have had the use and benefit of it during all that period, together with a big advantage over the man who saved; for this commodity would have 'appreciated' about four fold. As it was, his only reward of thrift was punishment. He had really lost, not only his interest, but also part of his principal. . . . In short, he put in 100 dollars and took out the equivalent of 80 such dollars." [8]

The reverse principal holds when the value of the dollar is increasing. Then it pays a man to put his money away—to hoard it—rather than to invest it in goods. The illustration of the shrinking dollar during inflation was written by Fisher in 1928. Suppose someone who read it at that time decided that he would put his money in goods instead of saving it. Instead of profiting from such a procedure, as he might have been led to believe, the value of his goods would have depreciated to about 60 per cent of the purchase price. If, however, he had put his money in a safe deposit box in October 1928, where it would have drawn no interest, he would have found, in March 1933, each dollar thus hoarded to be worth $1.65.

In spite of these facts, Fisher continues, we enter into contracts, debtor-creditor relationships, without regard for the prospective changes in the value of the dollar and after a period of years, either the creditor or debtor awakes to the fact that he has been cheated by the operation of dollar values.

Fisher is certainly right in his contention that changes in commodity prices (which is another way of saying that the value of the dollar changes) goes along with business cycles. He argues that, could we forsee these changes we would prepare ourselves and avoid these ill-effects. Such knowledge would help us little, however. Suppose that all of us knew that a period of deflation were at hand—that in five years everything that we owned would be worth just one-half of its present value—would we not all make a mad scramble to sell out and to put our money in some vault until we could spend

[8] Irving C. Fisher, The money illusion, Adelphi, 1928, pp. 67–68.

it to advantage? It would be a vain scramble, however, for there would not be enough money to go around. Or, if we were sure that money would depreciate in value, that there would be an inflation, so that in five years our present dollar would be worth fifty cents, would we not all spend our money as fast as we could and buy goods which we knew would be worth twice as much in five years? A wild rush to buy would result in such a stampede as was witnessed in Germany a few years ago.

Fisher makes a correct description of what actually happens as a result of price changes but he fails to unearth the cause for such changes. He is describing an important symptom, a symptom which we think is closely related to a psychological causal factor which we shall presently describe.

Although psychological elements are present in some of the preceding theories, the only theory that stresses a psychological factor in the business cycle is the optimism-pessimism theory. It was first proposed by John Mills, in 1867, in a paper called "Credit Cycles and the Origin of Commercial Panics." To him, credit inflation was an expression of optimism. He reasoned that fair trade breeds optimism; optimism breeds recklessness; recklessness breeds disaster; disaster breeds pessimism; pessimism breeds stagnation; stagnation lasts until men's spirits begin to recover because they discover that things are so bad that they cannot get any worse, or have gone less badly than they feared; fear having gone, they begin to do business again and the upward progress to another boom and excessive optimism is started. Here again we have a description of a symptom which is found in most business cycles but the theory fails to go beneath the symptom.

A Psychological Interpretation

This brief review of theories of business cycles shows that practically every symptom of the business cycle has been invoked to explain it. There are theories which put the blame on our capitalistic economy, some which blame our methods of exchange, some which blame our money economy, some which blame it on human psychology. All have facts in their favor but how is one to judge the relative value of them?

While these theories are so diverse that they leave the reader mystified, they have made two very important contributions. They have enabled us to differentiate sharply between organic and functional business psychoses. The older depressions came from external calamities such as earthquakes, plagues, crop failures, plunderings by hostile nations, and the like. Such depressions are not hard to understand. It is not hard to see why the South suffered a depression following the Civil War after she had spent all her money and had her slaves freed. It is easy to comprehend the misfortune of a planter who has his wheat crop destroyed by locusts.

A more important contribution from a study of these theories is the discovery that functional depressions are invariably preceded by business booms. It is clear that the boom and depression must be studied in conjunction as aspects of some central and related process which causes the whole cycle.

The next question that confronts us is the nature of the relationship between the boom and depression. Is it a causal relationship? Does the boom cause the depression or does the depression cause the boom? If both were true, we would have a species of perpetual motion. It is more probable that there is some hidden factor responsible for the entire cycle, or that the hidden factor causes the inflation and the inflation precipitates the collapse and depression.

With the exception of these two findings, the business cycle theorists have been mainly concerned with elaborate descriptions of the various symptoms of the business cycle. They have demonstrated by elaborate statistical studies that during a boom, prices rise, production increases, employment grows, debts increase, interest rates rise, optimism grows, and buildings multiply. All of these fall off in a depression.

There is a great temptation to fall into the error of thinking that an accurate description of these symptoms constitutes an explanation; but it is just as much of an error as to think we have explained a manic-depressive psychosis when we have made careful curves of changes in motor activity, rate of thinking, of emotional reactions, and of bodily temperature in the different cycles through which our patient passes.

A still greater error comes when one attempts to cure the disease by treating symptoms. This latter error we have witnessed in various forms and degrees during the last cycle. As we have said, various warnings were issued before the crash of 1929. The Federal Reserve Board tried to put on the brakes by raising the discount rate, by issuing warnings, and by selling securities. Such restraints were about as futile as applying strait-jackets and restraining devices to our manic patient. They may hold him down somewhat but they do not cure him and may even excite him to further manic behavior. Eventually the crash came and the Reserve Banks reversed their tactics. They lowered the discount rates and bought securities, both of which processes were artificial stimulants to business; but business declined only the faster. Their tactics were just as futile as the administration of artificial stimulants is to the depressed patient in the psychopathic hospital. Then attempts were made to cheer up the jaded nerves of business men by optimistic pronouncements by various leaders of political parties and business enterprises but with about as much effect as optimistic statements have upon a depressed person. Prosperity being around the corner became a joke. With each such pronouncement our business patient slumped the more. Then the group that had been imbued with the over-saving philosophy of Foster and Catchings had their inning and people were urged to spend and buy their way back to prosperity. This makes one think of the optimist who tells the depressed patient to get up and dance so that his gloom may vanish. Then we were told that we had produced too much so we turned under our crops and killed pigs on the theory that we must have rising prices to bring back prosperity. Such destruction, however, did not enable the jobless to buy what was left after the destruction. So we were told that the trouble was with the distribution of wealth and that, by taxation, we must extract the savings from the thrifty and give jobs to those who had nothing. But these who were given jobs must not produce or we would have another glut on the market so they must be given such useless jobs as scraping dirt on and off our highways. Finally, the symptom of too

little money had to be remedied by cutting down the content of the dollar. This latter treatment reminds the writer of the trick played by a father on his son who was in deep emotional depression because he wanted two pieces of candy instead of the one he possessed. The father broke the candy into two pieces and told the boy that he now had his wish—to dry his tears.

In the meanwhile, our business patient has been showing signs of recovery. Is the recovery taking place because of any one of these applications of theory, because of a combination of them, or in spite of them. We hear arguments for all three of these possibilities.

It may be necessary in an emergency to deal with symptoms. Business has been sick and anything that can be done to alleviate the pain may be justified. The danger comes when dealing with superficial symptoms becomes our goal.

Is it possible to get behind these diverse symptoms which manifest themselves in a manic-depressive psychosis of business and to discover the underlying factors which cause the disease? Perhaps we can get our answer, if we discover why men go to the speculative excesses which characterize a boom period.

The reason that is commonly given for the excessive optimism which leads to a business boom is lack of sound judgment. Men become blinded by false hopes and cease to be guided by their better judgment. Certainly the last boom was not accompanied by ignorance. Business men were aware that things were moving at too fast a pace. The newspapers and financial magazines warned that brokers' loans to the extent of eight or nine billions of dollars were excessive. The great amount of future income that was tied up with instalment buying was known to contain a dire threat to the financial soundness of the country. The Federal Reserve Board issued warnings, raised the discount rate, and sold securities in an attempt to curb the wild speculation, but in vain. Certainly the cause was not ignorance.

Business Mania Caused by Fear

We believe that it is fear that drives men to wild speculation, just as it is fear that drives men into manic flights in manic-depressive psychoses. Manic-depressive patients become emotionally elated because they are running away from some internal conflict which they dare not face and from which they have learned to defend themselves through distraction by trivial incidents of life, by excessive activity, by the excitement of running into uncertainty, and by forced happiness. While a close analogy may be dangerous, we think that a business mania may be interpreted in a similar fashion.

In the final stages of a boom, the wild speculator does not imagine that prices will go up forever, that business will continue to rise indefinitely. He may preach a "new-day" but his preaching is a whistling in the dark to allay his own fears. He knows that there will be an end to the boom, that the turning point will come but with each fear he plunges deeper, trusting to fate that he will get out just before the turning point comes. A boom is not a period of over-optimism; the optimism is an artificial device, an attempt to hide the underlying fear.

It is this fear factor which makes the crash so violent when it does arrive. Everybody gets "jittery;" the swings on the stock market become extreme, and those who think they are prudent place stop-loss orders. When the break finally comes these stop-loss orders pile up so fast that they cannot be executed. Everybody is wildly trying to sell and everybody afraid to buy. Sometimes it takes but the slightest shock to start the downward plunge, a situation which could not develop if everybody were optimistic, if the excessive activity did not hide an underlying fear.

Not all fears cause psychoses. A direct fear of some concrete danger is handled by dealing with the cause of the fear. As long as we are fighting a fear-producing situation, we are mentally sound; and if business could always be fighting in the open there would never be any business psychoses. There would be changes in the tide of fortune but there would not be insane manias and equally insane depressions.

What is this subconscious fear which lurks behind all business and produces psychotic symptoms? We believe that this fear is to be found in the shaky nature of our credit system. Such fears were *not* a part of the old barter system. Under such an economy each man had tangible evidence of his possessions. He either had stores of goods or he had their equivalent in money which, in itself, had intrinsic value and which he could be sure, at any time, was transferable into other goods. Business was done on a cash basis; and the cash that was used had intrinsic value. His only fears were of very evident dangers; such as having his goods or his money stolen or being worsted in a trade.

Today at least ninety per cent of our business is done on credit and what money we do have has no intrinsic value. It is simply the promise of the government to pay—not in gold or anything of value but in other dollars of the same sort. The entire business structure is built upon promises to pay.

Let us see how a small gold reserve held by a government can be used as a foundation for a very shaky credit pyramid. A government with, say, two billion dollars in gold issues paper money to the extent of four to six billion dollars, and bonds—which we are told are payable in gold—to the extent of ten billions more. These money tokens and bonds are placed with banks and represent security for commercial loans made to customers. The loans are not in actual money but in bank credits upon which the borrower issues checks. Personal checks issued on the basis of such loans are passed from buyer to seller and are, in effect, money. With only two billions of gold in some central storehouse, a country can do business which runs into hundreds of billions of dollars a year.

What a difference in the faith needed to operate such a credit system as we have and that manifested by the old traders each of whom held on to his possession until he had a firm grip on the object he was to receive and both dropped their hold upon one of the objects after counting: one—two—three!

The credit system is a system built upon faith, but faith in what or in whom? At this point we come to the element in

modern business which we believe to be the cause of the manic-depressive psychosis of business. It is a lack of insight, or a lack of candor, as in what or in whom we are actually putting our trust when we extend or accept credit. We have a belief that we are continuing to put our trust in gold in the same manner that we did when we traded for cash and received gold in payment. Our contracts are all written as though they would ultimately be paid in gold coin, but they are written in such excessive amounts that they can *never* be paid in gold coin. The fact that, in recent years, whenever there was a money stringency, every country involved had to suspend gold payments is enough evidence of that fact. We think we have money on deposit in banks, but, were all depositors to claim their money today, there would not be enough money in the world to pay them. It is illegal to possess a gold dollar at this time and yet we conduct business as though it were on a gold basis. This is the self-deceit which is the basis of the disintegration of business.

Yet without faith in something business could not go on at all. What is it that men trust when they do business? Their trust is in two things: First, that business will continue to go on. They have faith in the world as a going concern. Secondly, they have faith that men will, on the whole, fulfill the contracts they have made either through force of laws or through their own integrity.

If the only essential was to believe that business would continue, business cycles would be of minor extent. Business men would try to foresee all contingencies, and would adjust accordingly. They would make preparations for changes just as we make preparations for changes in weather based on all the information we can gather. With this one element operative, we would have business cycles but they would not be the extreme cycles which we have been describing.

On the other hand, our faith in men and in our modern institutions is not solid enough to resist fluctuations. Business oscillations are due to the ebb and flow of our faith in them. When we become skeptical of the integrity of our fellows or our government we want to run to the cover of a gold

standard; we want to contract our credit. When we see the government borrowing more than she can ever pay back, we begin to entertain grave doubts. Such a situation obtains, for example, after an expensive war. When municipalities, corporations, and private individuals all appear to be taking upon themselves more and more obligations, the fear and distrust of any far-seeing man will begin to accumulate.

But what can he do? He may have growing fears but he cannot stop. His very business existence depends upon continuing in activity, so he drives himself to more activity to meet the growing mistrust within him. The manufacturer makes greater and greater quantities of goods at thinner and thinner profits, hoping that he can get rid of his products faster than the other fellow and that he can pile up enough reserves to tide him through the lean years that he knows will come. He piles up a greater and greater cash surplus because he knows he must gird himself for the impending slump. This money is placed in reserve centers to be loaned to others who must expand their business because of the same fear motive. Goods must be somehow sold, so all sorts of time payments are arranged, and these depend on the hopes of the buyers that they can hold their jobs. But their very jobs depend upon the ability of producers to sell an increasing quantity of goods to an increasingly satiated public.

Booms are Defense Mechanisms

The boom is a mania, a defense mechanism, functioning to hide the ever-rising tide of fears. Fears drive to further activity and further activity increases the fear, moving in an upward spiral which becomes more and more unstable.

A boom is a psychological affair; it is a problem in human emotions which have grown to pathological proportions. Once a boom gets started it soon gets out of hand and must run its course in the same manner that a manic-depressive psychosis must run its course in the individual. That course is an inevitable mania with a reaction to deep depression. Cures cannot be applied during the course of such a cycle; all that can be done is to make the patient as comfortable as possible

until the disease has run its course. But after he is over the mania and the succeeding depression, steps can be taken to prevent a recurrence of similar attacks.

Economists are working out methods of measuring public confidence in their attempt to predict what is going to happen so that they may take advantage of the turns; but taking advantage of the turns is quite another problem from removing the causes of insane extremes. It reminds one of the way one group of men took advantage of the manic-depressive cycles of one of their major executives. They studied him so that they knew the signs of a change and could predict his spells of elation and depression. They organized the business so as to place him in strategic positions when he was in a manic mood; and he could accomplish miracles at such times. When they foresaw a depression they would put him at a task where he could do no harm until he recovered. Economists talk as though they would like to keep business in a state of continual mania. To them it seems too bad that there must be a depression and they think the mania causes the depression. We have tried to show that the depression does not cause the mania nor the mania the depression; both result from our unstable credit structure—the breeder of fear, and this fear causes both groups of symptoms in turn.

After the cause of a psychosis is found there are two ways of curing the malady. One is to provide a substitute defense; a sublimation, a better defense mechanism, or some better solution of the conflict. The other is to remove the cause—in this case the fear of insecurity because of distrust in the credit system.

Today the government is attempting to remove the distrust by changing credit from private and corporate hands to the hands of the government. To what extent they have gone in this direction may be seen from the fact that today 98 per cent of the Federal Reserve Credit outstanding is in government financing. Whereas in 1930 about equal amounts of Reserve Credit was invested in bills discounted and government securities; now 50 times as much is placed in government securities as in commercial discounts. In other words,

faith in the government is being used to offset lack of faith in industrial and business enterprises. When faith is restored in the ability of business to prosper the tide will turn. A danger in this procedure, it may be pointed out, is the well-known fact that the more any institution borrows the more it reduces its credit and the less faith it deserves. Our government may be able to stand the credit strain but it will have to pay off the debts in some fashion. Besides, most crises have been precipitated, in the first place, by unwise expansion of credit by governments on account of war-borrowings.

In other words, the present emergency measures are no more getting at the cause of the trouble than pouring tonics of iron, quinine, and strychnine into a depressed patient would be. We have blithely built a civilization on promises that our children and grandchildren would pay the bills. Then we try to forget the crime thus perpetrated on our unborn offspring by losing ourselves in manic flights and fits of depression. If we are ever to remove the cause, we need a group of leaders who are heroic enough to pay our bills instead of contracting more. The only final solution is an economy where—if the occasion arose—everybody could pay off his obligations in a very short space of time. There would then be no occasion for the hidden fear which now underlies all business that a day of accounting might come, nor the consequent desire to run to cover so as to beat the other fellow, driven by the knowledge that only the first few to arrive at the goal will remain alive economically.

Since this looks like an impossible utopian idea in the present state of affairs, the only other alternative is to adopt a defense mechanism different from the manic defensive episodes. We have no new defense mechanism or sublimation to suggest. We submit that this is a psychological problem and leave it to this group of psychologists to propose such a substitute.

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AN OBJECTIVE THEORY OF EMOTION [1]

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Theories attempting to explain the emotions are found in the very earliest psychological literature. The ancients considered emotions to be non-physical entities located in the various visceral organs. They believed that sorrow was located in the heart, jealousy in the liver, hate in the gall-bladder, anger in the spleen, etc. The more modern mentalists do not thus locate the emotions but define them no less psychically, as the "affective aspect of the operation of any one of the principal instincts." [2] For example, the instinct of escape is accompanied by the emotion of fear, the instinct of combat is accompanied by the emotion of anger, the parental instinct is accompanied by the emotion of love and tenderness, and so on through fourteen instincts and their accompanying emotions. In other words, emotion for the mentalist is the psychic aspect of the physical processes commonly known as instincts. A recent writer says that, "Emotion is the cognitive aspect of the physiological processes stirred up by a situation which is natively or habitually unwelcome or dangerous or unusual to the organism." [3]

Prior to 1885 the mentalist understood emotion to be a psychic phenomenon which aroused or caused physiological

[1] Max Meyer has pointed out in a stimulating article ("That whale among the fishes—the theory of emotions," PSYCHOL. REV., 1933, 40, 292–300) that emotion is fundamentally no different from other forms of behavior. A complete understanding of organic behavior will include all information about emotion. If it were possible to solve the problem of organic behavior adequately, there would then be no need to discuss emotion. It would have been included. The writer agrees with Meyer *if* that condition could be met. However, since we understand organic behavior only inadequately, it would seem justifiable to discuss emotion as a separate problem. It is perhaps too important to be submerged in a general discussion of organic behavior.

[2] Wm. McDougall, An introduction to social psychology, Luce and Co., 1926, p. 49.

[3] Max Schoen, Human nature, Harpers, 1930, p. 360.

condition, instinctive in character. However, William James and Carl Lange each developed independently the idea that the psychic state called emotion is the *result* of *previously* aroused physiological conditions. The James-Lange theory is too well known to require further elucidation.

Cannon [4] raises a number of vital objections to the James-Lange theory. First, experiments show that a "total separation of the viscera from the central nervous system does not alter emotional behavior." Sherrington [5] was the first to sever neural connections between the viscera and the brain (using dogs as subjects) and found that it had "little if any effect on the emotional responses of the animals." Second, "the same visceral changes occur in very different emotional states and in non-emotional states. . . . The responses of the viscera seem too uniform to offer a satisfactory means of distinguishing emotions which are very different in subjective quality. Furthermore, if emotions were due to afferent impulses from the viscera, we should expect not only that fear and rage would feel alike, but that chilliness, hypoglycemia, asphyxia, and fever would feel alike." Third, "the viscera are relatively unsensitive structures. . . . Surgeons have found that the alimentary tract can be cut, torn, crushed or burned in operations on the unanesthetized human subject without evoking any feeling of discomfort." Fourth, "visceral changes are too slow to be a source of emotional feeling." It is a well known experimental fact that the reaction time of smooth muscles is much slower than that of striped muscles. Visceral changes do not occur soon enough to account for emotional behavior. Fifth, "artificial induction of the visceral changes typical of strong emotions does not produce them."

Thus it would seem that we must abandon the James-Lange theory as being inconsistent with scientific fact. Let us now investigate the theory of emotions developed by Can-

[4] W. B. Cannon, Bodily changes in pain, hunger, fear and rage, Appleton, 1929, Chap. 19.

[5] C. S. Sherrington, Experiments on the value of vascular and visceral factors for the genesis of emotions, *Proc. Roy. Soc.*, 1900, p. 397.

non himself and which, he claims, is in harmony with all the known facts.

Experimental evidence seems to show that the thalamus is "a region from which, in the absence of cortical government, impulses are discharged which evoke an extreme degree of 'emotional' activity, both muscular and visceral." [6] In fact, continues Cannon, "The neural arrangements for emotional expression reside in the subcortical centers, and these centers are ready for instant and vigorous discharge when they are released from cortical restraint and are properly stimulated." [7] These inferences lead Cannon to state the theory that "*the peculiar quality of the emotion is added to simple sensation when the thalamic processes are roused.*" [8] In other words, instead of explaining emotion as mental processes which precede bodily behavior (as did the early mentalists) or as mental processes which follow bodily behavior (as did James and Lange), Cannon has explained emotion as being mental processes produced in the thalamus which may attach themselves to the neural processes of bodily behavior. Cannon does not use quite the same terminology as other mentalists, yet he eliminates the possibility of a non-mentalistic interpretation. He says, ". . . thalamic processes may add to sensation an aura of feeling." [9] Again he speaks of emotion as being a "feeling tone." It would seem that Cannon's main contribution over that of James and McDougall has been the experimental localization of the part of the brain necessary for emotion. Cannon has proven that the diencephalon is essential for emotional behavior.

The objection to Cannon's theory of emotion is similar to the objection to the mentalistic explanation of anything—it does not explain. He merely pushes the mystery back a step and calls it a "feeling tone." But what is a feeling tone? What is a "cognitive aspect"? To name a problem is not to explain it. Furthermore, in common with all mentalistic

[6] W. B. Cannon, *op. cit.*, p. 362.
[7] *Ibid.*, p. 368.
[8] *Ibid.*, p. 369.
[9] *Ibid.*, p. 370.

theory, Cannon has no evidence to prove that the thalamus adds feeling tone. He has only demonstrated that the higher centers are not essential to emotion. His evidence proves that the thalamus is probably the neural center which functions in emotion, but he has not proven that a feeling tone or anything else is *added* to the neural flux as it passes through the thalamus. He has not proven that the thalamus does more than act as a distributor of neural flux. In fact, experimental evidence indicates that a neural impulse is not altered in the central system at all. Herrick calls the central system a "switchboard mechanism" which merely distributes neural flux to the various motor points.[10] He expresses the opinion that future scientists will find it possible "to write chemical and energy equations to express the physical changes in my cerebral cortex while I solve a mathematical problem and to map the course of all of the nervous currents which may play back and forth in my brain." [11] Newman says, "The brain is something like a telephone exchange, where communication is established between numerous parts of the organism." [12] There is no evidence that any neural center does more than convey neural impulses from afferent to efferent pathways.

Therefore, while we must accept Cannon's experimental evidence as being scientific and indicative of certain facts concerning emotion, we cannot accept his theory of emotion as being anything but another bit of mentalistic speculation. Cannon has resorted to vitalistic postulation which is not supported by his scientific evidence and, although carefully camouflaged with experimental procedure and terminology, it is but little in advance of the theories of James and McDougall. We must conclude with Wheeler that "Cannon has not proven that the feeling tone is supplied by the thalamus." [13]

Now can emotion be explained on a purely objective basis? Perhaps a review of some of the more evident physiological processes which take place during that state of existence con-

[10] C. J. Herrick, The thinking machine, Univ. of Chicago Press, 1929, Chap. 8.
[11] C. J. Herrick, Brains of rats and men, Univ. of Chicago Press, 1926, p. 19.
[12] H. H. Newman, Outlines of general zoology, Macmillan, 1924, p. 281.
[13] R. H. Wheeler, The science of psychology, Crowell, 1929, p. 166.

ventionally designated as emotional will help to clarify the problem. The average man consumes approximately 3000 calories during a twenty-four hour period of normal living. He uses less during his sleeping hours than during his waking hours and a great deal more during his active hours than during his semi-active hours. In extreme exertion he may consume calories at the rate of 5000 per hour. Since the body tissues do not contain reserve materials for more than basal metabolic consumption (about 100 calories per hour), the blood must supply it as it is needed. It has been found that the sugar (glucose) content of the blood is constant (.12 to .15 per cent), which means that the liver must manufacture blood sugar (convert glycogen to glucose) at a variable rate. In other words, because behavior varies (and therefore releases energy at varying rates), the food products consumed during the process likewise vary in amount. As the blood and body tissues maintain a constant supply of glucose, the liver must regulate its rate of production as determined by the body's rate of consumption. This means that some control mechanism must be sensitive to the chemical condition of the blood and somehow control the function of the liver. Cannon [14] has found this to be the adrenal gland. He analyzed the blood of cats during intense activity and found that it contained an increased amount of adrenalin. He also injected adrenalin into the blood stream and found that the function of the liver was accelerated. Without question, then, the adrenal gland is the regulator of the liver and explains why that organ manufactures blood sugar only as it is needed.

However, the presence of adrenalin in the blood also produces other bodily effects—erection of hair, increased heart beat, heightened muscle tonicity, dilation of the pupils, increased blood pressure, increased breathing, etc. Various combinations of these secondary effects of adrenalin make up the characteristics of the various emotions. Stated differently the bodily effects of adrenalin are classified and called by the names of the several emotions. We hasten to add, however, that adrenalin is not the only hormone that functions during

[14] W. B. Cannon, Bodily changes in pain, hunger, fear and rage, Appleton, 1929, Chap. 3 and 4.

emotional states. The products of the other endocrines also have regulatory value. Weiss says, "The whole system of the glands of internal secretion may be regarded as a regulating mechanism by which the nervous and muscular tissue is being prepared to meet the energy demands of certain stimulating conditions." [15]

The facts stated so far would perhaps be admitted by all mentalists. But they insist that these are only the obvious facts of emotion. What about the "feeling tone," the "consciousness," which is necessary to make this behavior truly emotional? What about the subjective experience or the emotion *per se?* The writer believes that this, too, can be given scientific and non-mental explanation.

There is no question that the adrenals function lively during the emotion of fear and rage, and that the gonads (as endocrines) are very active during the emotion of love. But as Berman explains,[16] the endocrines all function in cooperation with each other, or as an "interlocking directorate." It is inaccurate to say that any specific emotion is due to the function of a certain endocrine. It is perhaps more accurate to say that all the endocrines function during all emotions but in different proportions. The adrenals, for example, are more active during rage than during love. Each emotion has its characteristic endocrine function pattern and consequently its characteristic blood chemistry formula. The blood chemistry in rage differs from the blood chemistry in love, and the blood chemistry in fear differs from that in either rage or love. The reason is that the endocrines behave differently for these respective emotions.

Now, if the chemistry of the blood during emotional behavior varies from the normal, it is quite plausible that the proprioceptors located in the vascular system would be stimulated accordingly. And if each emotion is characterized by a distinctive blood chemical composition, certainly the proprioceptors would be sensitive to the difference. One blood chemical composition (that for fear) will stimulate the proprioceptors differently than another blood chemical com-

[15] A. P. Weiss, A theoretical basis of human behavior, Adams, 1925, p. 371.

[16] L. Berman, The glands regulating personality, Macmillan, 1928, Chap. 4.

position (that for love). Thus the *feeling* of fear is the internal stimulation aroused by the individual's own blood chemistry. He may behave in a way which would cause an observer to report fear but he will *feel* afraid only when his blood chemistry has changed to the formula characteristic of fear. The "feeling tone" becomes, therefore, a proprioceptor stimulation caused by the chemical composition of the individual's own blood. The way he feels is determined by his own blood chemistry.

There is ample evidence to substantiate this view of emotional *quale*. First, that the vascular proprioceptors are sensitive to blood chemistry is substantiated by the opinion of outstanding authorities.[17] Stiles of the Harvard Medical School says,[18] "As to the question whether nerve-endings are excited as a result of changes in the chemical composition of the blood, there seems to be every reason for assuming that this is constantly happening." The same author states in the New England Journal of Medicine,[19] "Evidence has been secured that the carotid sinus contains chemical as well as mechanical receptors. Changes in the composition of the blood passing through this arterial segment may alter the rate and depth of breathing." Carlson of the University of Chicago says,[20] "Both the peripheral nerve endings and the ganglia as well as the central nervous system are sensitive to changes in the composition of the blood. . . . A great many substances in addition to adrenalin act on peripheral nerve endings. . . . Carbon dioxide in sufficient concentration acts peripherally and centrally on the nervous system." Mitchell of Brown states, "There is abundant evidence obtained from intravascular injections of hormones, drugs and toxins to show that many of the familiar effects thus obtained involve nerve excitation." [21] Greene of the University of Missouri says,

[17] As the biological literature on this point was indefinite, the author wrote to a number of prominent specialists in the field of vascular physiology and bio-chemistry, and asked the question—Do changes in the chemical composition of the blood initiate neural impulses in the vascular proprioceptors?

[18] Percy G. Stiles, in a letter to the author, May 3, 1934.

[19] For March 2, 1934, pp. 492–494, "Recent progress in physiology."

[20] A. J. Carlson, in a letter to the author, April 30, 1934.

[21] P. H. Mitchell, in a letter to the author, May 10, 1934.

"We physiologists look upon the hormone of the adrenal medulla as a specific substance reacting with the myoneural terminations of not only the vascular autonomics arising from the thoracic cord, but with end-organs of various other so-called sympathetic pathways." [22]

Second, the recent experiments by Landis and Hunt [23] prove that subcutaneous injections which change blood chemistry do arouse varying degrees of emotional states. These investigators used psychiatric patients as their subjects and carefully noted the effects of two types of injections—1–1000 solution of Parke, Davis Adrenalin Chloride and a control solution of chloretone and physiological salt. Among the twenty-seven subjects used, only one showed any emotional reaction to the control injection, whereas in every case the adrenalin injection aroused various types and degrees of emotional states. These results are substantiated by Cantril and Hunt in their earlier experiments with normal subjects.[24] From the results of the two investigations Landis and Hunt conclude that "In general, the injection of sufficient amounts of adrenalin will reproduce roughly the organic picture usually characterized as emotion. There is a considerable variation, both quantitative and qualitative, within the specific details of this picture. The production of this organic state characteristic of the adrenalin syndrome gives differing results. Some subjects report merely the organic syndrome. Some report an associated emotional content, saying they feel 'as if afraid' or 'as if in great joy', etc. In a few cases a complete emotion is present." [25] Thus it would seem that we can conclude without question that changes in blood chemistry do initiate neural impulses which are interpreted by the individual to be emotional feelings.

This theory explains the evidence which caused James and Lange to arrive at their theory of emotion. The individual is

[22] C. W. Green, in a letter to the author, May 3, 1934.

[23] Carney Landis, and W. A. Hunt, Adrenalin and emotion, PSYCHOL. REV., 1932, 39, 467–485.

[24] H. Cantril, and W. A. Hunt, Emotional effects produced by the injection of adrenalin, *Amer. J. Psychol.* 1932, 44, 300–307.

[25] C. Landis and W. A. Hunt, *op. cit.*

stimulated by an emotional situation but does not *feel* the emotion until after his endocrines have functioned sufficiently to change his blood chemistry. The emotional feeling really does follow changes in physiological condition. As endocrinology had not yet been developed at the time James and Lange formulated their theory of emotion, it was quite excusable for them to assume that the feeling of emotion is mental in character and is brought about not by the emotional stimulus, but by the physiological responses to it. It has always been characteristic of mentalistic psychology to confuse internal stimulation with postulated vitalistic phenomena. James and Lange likewise erroneously attributed the feel of an altered blood chemistry to mentalistic functioning.

This explanation also accounts for the fact that emotion is felt "all over." Certainly the individual would feel angry all over because he is feeling his own blood. It explains why various parts of the body can be anæsthetized and still the individual feels emotional. Even Sherrington's neurally decapitated animals could receive stimulation from the vascular proprioceptors located in the head, and therefore feel emotional.

Thus, emotion can be explained objectively as the intense but temporary bodily behavior initiated by some sort of stimulation sufficient to bring about immediate action of the endocrines (and a resultant change in blood chemistry), profound visceral activity, heightened muscle tonus, increased sensitivity, etc. These physiological changes (especially the changes in blood chemistry) then stimulate the internal receptors and the organism *feels* emotional. He feels himself behaving in that way called emotional. Different emotional feelings are distinguished because blood chemistry and other physiological changes are so characteristically different for the various emotions that the proprioceptors are stimulated differently. This is no psychic state but physical behavior in response to internal stimulation.

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GOAL GRADIENT OR ENTRANCE GRADIENT?

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In the analysis of maze errors of the rat a great deal of emphasis has been placed upon the relative distance of the culs-de-sac from the goal. Among recent contributions, Hull's goal gradient theory (9) and Spence's summary of the literature (10) are well known. Both of these authors consider the distance of an alley from the goal one of its most important attributes. The facts which are brought to support this view consist chiefly of data which show a decrease in difficulty of blinds from entrance to goal when other factors are to some extent equated. Now while a gradient of alley difficulty may exist between the entrance and the goal, this gradient does not reveal the source of its own cause. May it not proceed from the entrance? It is not my purpose to criticize the goal gradient theory in its entirety, or to deny that the goal has an effect upon alley difficulty. I do wish to show that data exist which indicate that under some circumstances the proximity of the entrance is more important in causing the first alleys of mazes to be difficult than is the remoteness of the goal. This is, I believe, a neglected fact.

The experiments which demonstrate this statement most clearly are those of Warden and Cummings (11) and de Montpellier (3). In these studies the maze patterns consist in a series of simple alternations. Half of the culs lie to the right and half of them to the left of a line between the entrance and the goal. Both of the investigations used, among other lengths, a ten-alley maze pattern. When the results of the two ten-alley mazes are combined, successive right-left pairs of alleys from entrance to goal induced the following total errors: 320, 128, 57, 67, 90. The two researches give very similar data, as does an experiment of my own using the same simple alternation pattern (5).

The problem with which the present paper is concerned is as follows: Is the great decrease in difficulty in the first, second, and third pairs of alleys (the first six alleys) due to the increasing remoteness from the entrance as the maze is traversed or to the increasing nearness to the goal? This question, it seems to me, may be answered by comparing the data from the ten-alley mazes with the data from simple-alternation mazes of other lengths.

In the experiments from which the above data were taken use was made of a six-alley maze under the same conditions as the ten-alley maze. Adding the data as before, the total errors in successive right-left pairs of alleys are 227, 95, 76. The absolute number of errors need not concern us, as the problem is usually stated in terms of *relative* difficulty. It is most significant that the three pairs of alleys of the six-alley maze were placed in identically the same relation to the goal as the last three pairs of the ten-alley maze, yet an increase in difficulty as the goal was approached occurred in the latter and a great decrease in difficulty occurred in the former. Furthermore, the six alleys of the short maze bore the same relation to the entrance as the first six alleys of the long maze. If the relation to the entrance is important, one would expect the relative difficulty of successive pairs of blinds to take the same course in the two situations. This is what occurs. In terms of per cent of total errors in the first pair of blinds, the first three pairs of alleys of the long and short mazes respectively were of the following order of difficulty: 100 per cent, 40 per cent, 18 per cent, and 100 per cent, 42 per cent, 33 per cent. The only observable effect of the greater nearness of the goal to the alleys of the short maze in comparison with the first six of the long maze is the slightly greater relative difficulty of the third pair of blinds in the short pattern.

It is not necessary to limit such comparisons to mazes of the lengths just discussed. Warden and Cummings used a two-alley maze of the same units and of the same pattern as the longer ones. These two alleys in isolation resemble in their error records the first two alleys of long mazes much more than they resemble alleys which adjoin the food boxes of the longer mazes.

I may repeat that it is not proposed that proximity of the goal has no influence whatsoever upon the relative difficulty of blind alleys. That it probably does have some effect is shown by the fact that the short mazes do not produce results which are entirely identical with the early sections of long mazes. The conclusion seems inescapable, however, that in simple-alternation mazes the characteristic order of difficulty of the early alleys is associated with their proximity to the entrance and not with their distance from the goal.

The explanation of the apparent influence of the entrance is not to be sought in some influence emanating from the starting box itself, but rather, I believe, in factors associated with distance from the entrance. One possible factor lies in the detrimental effect of handling and of removal from the home cage upon learning. Higginson (**8**) has shown that stimuli designed to elicit emotional reactions retard the learning of a maze by a rat. It may be that ordinary maze procedure elicits a lesser physiological upset which subsides with duration of time following entrance into the maze. Such a factor might be expected to produce the results which have been obtained.

A second hypothetical explanation of the progressive decrease in difficulty of maze alleys comes from the concept of transfer of training. In many maze experiments transfer occurs even between dissimilar mazes. It is apparent that when the rat enters the second half of one maze it has had more maze training than when it left the entrance. A positive transfer from the first to the second half of the maze might be expected.

One possibility of transfer lies in the transfer of the negative adaptation of exploratory tendencies. Recent studies have shown (**6, 7**) that the negative adaptation of exploration may be thought of as a basic process in error elimination. If the earlier and the later blinds have some stimulus-equivalence the exploration which occurs in the early alleys might prevent entrances into the similar blinds of subsequent maze sections.

Lastly the transfer between parts of the maze might be thought of as a transfer of a more positive reaction. For ex-

ample, the goalward orientation of the animal might be expected to become stronger and stronger if more and more stimuli for its assumption are met by the animal as it proceeds through the maze. This suggestion is not, as it may seem, a goal gradient hypothesis in another form, for it suggests that an orientation becomes more pronounced in later sections not because the stimuli to the orientation which are received at a point nearer the goal are more effective in themselves but because a greater number of stimuli tending to establish orientation have been received when the rat is near the goal than when it has just entered the maze.

Note

Since the above paper was accepted for publication, Hull (*J. Comp. Psychol.*, 1934, 17, 393–422) has presented a very interesting research on "The Rat's Speed of Locomotion Gradient in the Approach to Food." I am gratified to find that he is to some extent in agreement with the interpretation here proposed. He writes (p. 419): "The gradient of a 20-foot runway has the appearance of a fore-shortened gradient from a 40-foot runway, rather than the posterior half of a 40-foot gradient which should be the case if the goal excitatory gradient alone were operative. This apparently means that the speed of locomotion at a given point is not determined entirely by its distance from the goal."

It seems to me, however, that Hull has overestimated the influence of the goalward as opposed to the entrance end of the apparatus. Within both the 20-foot and the 40-foot pathways the difference in time between the first and second sections of the apparatus is greater than the time difference between any two consecutive subsequent sections. The greatest reduction in time occurs a short distance from the entrance regardless of the remoteness of the food box. While Graphs 4 and 5 of Hull's paper show that on the first and second days of training and on runs after frustration the first fifteen feet of the 20-foot pathway were run in less time than the same distance in the longer pathway, the relative reduction in time is the same and no other absolute differences appear on these graphs. On training days 6 and 7, practically no absolute differences appear in the common pathway units in spite of the fact that each unit in the short pathway is 20 feet nearer the goal than the same unit of the long pathway. Upon these same days there is still a distinct gradient within the first three sections. On that account, it cannot be said that the distance from the goal is ineffective simply because all gradients have disappeared. Similarly, there is no difference in time required in section 4 of the two pathways upon days 1 and 2, upon days 6 and 7 or after frustration, although this section of the short pathway immediately preceded the goal while in the long pathway it was separated from the goal by 20 feet of runway. Such similarity between the short pathway and the *first* half of the long pathway is to be expected, however, if one assumes that the most potent factor controlling running speed at any point is the time elasping since leaving the entrance compartment.

Hull introduces (p. 418) the question of the excitation gradient which may be necessary to account for the speed gradient of the 40-foot alley. He seeks to show that while the speed from entrance to goal may be plotted approximately as a straight

line with an upward slant, it seems likely that the excitation required to produce the regular speed increase is positively accelerated as he previously postulated. He reaches the conclusion that " without much doubt the excitatory gradient responsible for the speed gradient shown in figure 9 is one of positive acceleration *as the goal is approached.*" (Italics mine.) The records of rats on the 20-foot pathway are here neglected. These animals were nearly as slow as were the rats which were running the first half of the long path and show the same rate of increase in speed with distance. Now if the excitation gradient (whatever it is) is steeper *near the goal,* the increase in speed in the 20-foot alley should be steeper than the increase along the second half of the 40-foot pathway, since the "vigor of the muscular reaction" upon which this excitation works is weaker at the beginning of the 20-foot pathway than at the beginning of the second half of the 40-foot alley. In other words, the hypothetical excitation gradient also fits the data much better if it is measured from the entrance of the apparatus than if measured backward from the goal. If one supposes that there is a temporal course of recovery from the effects of being placed in a non-cage situation (the entrance) and a temporal course for the acquisition of maximum speed in running after leaving the entrance, there need be no assumption that the remoteness of the goal is of much importance for speed of locomotion.

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[MS. received June 5, 1934]

DISCUSSION

INTERPRETATION OF PROFESSOR SPEARMAN'S COMMENTS

I am unable to repress a few brief remarks on Professor Spearman's recent stern comments [1] on my treatment [2] of his two factor theory.

The first would be to suggest that, though I claim no infallibility, Professor Spearman possibly ascribes *too much* worthlessness to my treatment. His discussion contains such animadversions as: ". . . there is nothing correct in it"; "It is indeed strange for anyone to discourse on statistics who does not know . . ."; "If our author will here too kindly look again at what he criticizes . . ."; "He has touched nothing which he has not vitiated." I hope that I may not be accused of unfairness if I insist that these *ad hominem* strictures do not add strength to his criticism.

I find that Professor Spearman is more or less specific in his first criticism, which seems to be about as follows: In my first article, he avers, I erred by presenting as his version of his theory one which excludes group factors, and in the second, I then inconsistently admitted to his theory as one of its proper parts the postulation of such factors. That I should have led him to this misconstruction is most confounding, and I hasten to review at once the general, albeit simple, thread of my reasoning. My first article consisted in the main of a tetrad analysis of ten sets of statistically *adequate* data in part chosen (and after much searching) from the very large amount of material published by Professor Spearman and his coworkers. The purpose was to see with my *own* eyes whether the two factors alone could be postulated. In this I was indeed following the first step which Professor Spearman himself takes in his own tetrad analyses. I found no evidence of the two factors. The next step was to consider the possibility of group factors in addition to the alleged general factor and specifics. That Professor Spearman should have surmised that I ignored his admission of group factors is most disconcerting, especially in view of the fact that throughout

[1] C. Spearman, PSYCHOL. REV., 1934, 41, 306–307.

[2] R. C. Tryon, PSYCHOL. REV., 1932, 39, 324–351 and *Idem*, PSYCHOL. REV., 1932, 39, 403–439.

the first article I frequently mentioned the point and then, lest it escape the attention of a careless reader, I featured it prominently in the conclusions, thus:

> How does the two-factorist reconcile such results to his theory? He attempts to do so by keeping his finger on his *g* and his *s*'s and then he introduces a few factors or 'disturbers' which he claims are sufficient to explain the excess divergence of the tetrads from zero. This claim we shall consider in the paper which follows (p. 349).

In the second article I then took the two-factorists' *own list* of group factors which they themselves had postulated as adequate to account for the excessively large tetrads, and, to quote from the conclusions:

> . . . made an intimate analysis of these tetrad differences and . . . observed in practically no instance that the too large tetrads behaved in the systematic fashion required by these group factor postulations (p. 437).

I see now that I should have italicized these sections so that, to the veriest neophyte, the elementary run of the reasoning would have been utterly plain, to wit, that in adequate data, when analysed by the tetrad difference method,[3] one is quite unable *to prove* the existence either of the simple two factors *or* of these factors plus the group factors postulated by Professor Spearman and his students.

There were other generalizations to which I had hoped to draw the attention of the two-factorist. From the three score pages of these, Professor Spearman chose one bit upon which, as his second and last criticism, to exercise his analytical trenchancy. From my first article *he chose a footnote!* Now the seeming importance of this footnote lies apparently in the fact that it contains an error in *grammar!* Though I confess to the capacity of delivering worse linguistic atrocities than this error, in this particular instance, singularly enough, the error was not mine but the printer's.[4] Apart from such a picayune, the footnote is deemed also in error by Professor Spearman on other grounds. It deals with an important proposition frequently reiterated in his writings and one which aims to disprove the theory of a large number of independent factors as the determiners of individual differences. This proposition Professor Spearman expresses as follows:

> . . . a large number of random factors would reduce the scatter of the individual differences toward zero (p. 307).

[3] A method which in comparison with the refined mathematical techniques of Kelley, Thurstone, and Hotelling, proves to be singularly crude.

[4] In the author's manuscript and the first galley 50679 No. 14, the correct expression, "one die," appeared, but in the recasting of the footnote for another error a new printer's error, "one dice," slipped in. *Ed.*

The reader, if he has had an elementary course in statistics, may test this proposition himself. He will recall that the scatter of individual differences usually refers to the absolute *dispersion* of the scores of individuals. One very crude measure of dispersion is the *number of classes* or categories in which these scores fall. Consider the proposition with reference to this measure. Let the number of factors be n coins, and each individual's total score be the number of heads showing. The frequency distribution of such scores may be found from the expansion of the point binomial, $(p + q)^n$, where p refers to heads, q to tails, and n to the number of factors. If n is 1, there are two classes of individuals; if n is 2, there are three; if n is 3, there are four; that is, there are always $n + 1$ classes. Therefore, the larger the number of random factors, the greater the scatter of individual differences.

I see now that I should have treated the proposition in the above elementary way. Instead, in my footnote, I used the slightly more complex measure of scatter, namely, the *variance* (σ^2) of total scores. There it was shown that if X is the total score determined by n factors, each factor having the variance, σ_d^2, then $\sigma_x^2 = n\sigma_d^2$. Here, again, the greater the number of factors, the greater the total absolute variance or scatter. In his criticism, Professor Spearman suggests that σ_x^2 should be set equal to unity, his rationale being that statisticians commonly do so (p. 307). But to set the measure of scatter equal to a constant, when it is the *change* of this measure due to variation in n which is under consideration, is a mode of reasoning of which no competent statician would like to be accused. Had Professor Spearman suggested that the variance of each factor, σ_d^2, be set to unity, then this would have made sense, for then $\sigma_x^2 = n$, whence the fact again appears that the absolute scatter of individuals varies directly, not inversely, with n.

Some of Professor Spearman's confusion on this issue lies in the failure to discriminate between *absolute* and *relative* scatter. His measure, σ_x^2/X, proposed as a test of his proposition, is a measure of scatter relative to the magnitude of the total score earned by the individual. It is singularly unwieldy as such a measure, since for a given population, it takes N values. A more refined formula would have been $V = 100\sigma_x/M$. In our coin set-up, $V = 100\sqrt{npq}/np$, which tends to zero as n becomes large. Thus it appears that as the number of factors increases, the absolute scatter, σ_x^2, becomes larger, though the relative scatter, V, becomes smaller. But students of mental measurement are interested in absolute, not relative,

variance. In an actual test situation, the standard deviation, which is the *observed* measure of scatter, is a function of this absolute factor variance. Our conclusion therefore was this: the greater the number of factors, the greater the factor variance, and *pari passu* the greater the observed scatter of individual differences.

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